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CONTENTS.

I. *On a Possible Explanation of the Adaptation of the Eye to Distinct Vision at Different Distances. By* JAMES D. FORBES, *Esq., F.R.SS. L. & E., Corresponding Member of the Institute of France, and Professor of Natural Philosophy in the University of Edinburgh*, . Page 1

II. *On the Modification of the Doubly Refracting and Physical Structure of Topaz, by Elastic Forces emanating from Minute Cavities. By* SIR DAVID BREWSTER, *K.H., D.C.L., F.R.S., and V.P.R.S., Edin.*, 7

III. *On the Existence of Crystals with different primitive forms and physical properties in the Cavities of Minerals; with additional Observations on the New Fluids in which they occur. By* SIR DAVID BREWSTER, *K.H., LL.D., F.R.S., and V.P.R.S., Edin.*, . . . 11

IV. *Account of Experiments upon the Force of the Waves of the Atlantic and German Oceans. By* THOMAS STEVENSON, *Civil-Engineer, Edinburgh.* Communicated by DAVID STEVENSON, Esq., . . 23

V. *On the Geology of Cockburnlaw, and the adjoining District, in Berwickshire; with a Map and Sections. By* WILLIAM STEVENSON, *Dunse.* Communicated by DAVID MILNE, Esq. 33

VI. *On the Extraction of pure Phosphoric Acid from Bones, and on a new and anomalous Phosphate of Magnesia. By* WILLIAM GREGORY, *M.D., F.R.S.E., Professor of Chemistry in the University of Edinburgh*, ... 47

VII. *Miscellaneous Observations on Blood and Milk. By* JOHN DAVY, *M.D., F.R.S., Lond. and Edin., Inspector-General of Army Hospitals, L.R.*, 53

VIII. *On the Advantages to be derived from the Use of Metallic Reflectors for Sextants and other Reflecting Instruments; and on Methods of directly determining the Errors in Mirrors and Sun-Shades used in Reflecting Instruments.* By JOHN ADIE, Esq., ... 61

IX. *On the Balance Magnetometer, and its Temperature Correction. By* J. A. BROUN, *Esq.* Communicated by SIR T. M. BRISBANE, *Bart.*, 67

X. *On Wollaston's Argument from the Limitation of the Atmosphere, as to the Finite Divisibility of Matter.* By GEORGE WILSON, M.D., *Lecturer on Chemistry*, ... 79

TRANSACTIONS.

I.—*On a Possible Explanation of the Adaptation of the Eye to Distinct Vision at Different Distances. By* **James D. Forbes**, *Esq., F.R.SS.L. & E., Corresponding Member of the Institute of France, and Professor of Natural Philosophy in the University of Edinburgh.*

[Read 16th December 1844, and 6th January 1845.]

It is unnecessary to detail to this Society the various ingenious hypotheses which have been proposed to account physiologically for the accommodation of the eye to distinct vision at different distances. In later years, these different theories have been so circumstantially and correctly recapitulated in systematic works (as for instance in Young's Lectures and in Müller's Physiology), that it would be a waste of time to copy and recite them here. I will only do so, then, so far as may be necessary to justify the attempt I have now to make, and to strengthen my views by those of others, as far as they bear upon them.

The eye being the organ of sense best understood, and constructed upon the most intelligible principles,—being one whose functions, up to a certain point, may be accurately represented by an artificial apparatus, it is impossible to doubt that the ultimate function of vision depends on the formation of a distinct picture of an object upon the retina, and that the circumstances which affect the distinctness of the picture in the instrument or artificial eye, must affect the clearness of vision in the real eye. Such a circumstance is notoriously the distance at which objects are placed from the eye. Now it is known by experience, (1.) That objects at very variable distances may, in the healthy organ, be distinctly seen; (2.) That such variations have limits, beyond which distinct vision cannot, by any effort, be obtained; (3.) This limit varies in different eyes; (4.) The limit may be extended by optical aid, which would, in the model or artificial eye, produce the same effect; (5.) The adjustment of the eye to different distances is felt to be accompanied by a distinct muscular effort. On all these grounds, we conclude that the focal adjustment of the eye is a real mechanical adjustment, tending to form an optically distinct picture on the retina; and that the opinion of those physiologists is to be disregarded, who have supposed that the distinctness of vision at one distance or another arises merely from a mental effort of attention.

We assume it, then, to be granted that the adjustment of which we are in quest is of a nature such, that when the eye is turned from a distant to a near object, *either* the retina is moved from the refracting apparatus of the eye, so that the less convergency of the rays may be allowed for by the increased distance; *or else*, that the distance of the retina or pictured screen remaining the same, the refraction of the eye is increased, so as to cause the rays to converge more rapidly than they would have done in the previous state of the eye.

If the first alternative be true, the axis of the eye must undergo an *elongation* of about one-seventh part (according to OLBERS* and YOUNG†), in passing from the distinct vision of distant to that of very near objects. Dr YOUNG has described two experiments, by which, he says, he satisfied himself that the elongation of the eye could not be anything like the quantity required by the hypothesis. Dr YOUNG's experiments are obscurely described; but perhaps a not less conclusive and more simple proof of the error of this explanation is found in the fact insisted on by TREVIRANUS and MÜLLER, and which seems to me quite unanswerable,—"that the tendency of the straight (*recti*) muscles is merely to retract the eye, and if resistance were afforded by the cushion of fat behind it, to flatten rather than elongate it; their action would, therefore, have the effect of adapting the eye to the vision of distant objects only, the image of which is formed nearer the lens, than that of near objects; while it is in looking at very near objects, on the contrary, that we are conscious of an effort within the orbit."‡

It seems difficult to admit with MÜLLER, however, that any conclusion as to the mechanism of the eye can be drawn from the transient and anomalous changes of adjustment which it seems to undergo under the influence of narcotics, such as belladonna.

The other class of explanations turn upon the production of an increased refractive power in the eye, by the altered curvature of one of its numerous refracting surfaces. Every one of these has been in turn fixed upon as the subject of the change, as well as those parts of structure which, by their intimate connexion with the principal parts, might be supposed to influence them. The cornea, the lens, the iris, and the ciliary processes, have each been supposed to be the part immediately affected. Most of the theories have been refuted with consummate skill by Dr YOUNG, in his paper on this subject in the Philosophical Transactions for 1800; and, as is well known, he himself attributed the change of focal adjustment to a *proper muscular power residing in the lens*. This otherwise probable opinion is contradicted by the fact, that the muscularity of the lens is unproved, and that this organ is wholly unprovided with bloodvessels and nerves. The opinion now adopted by several eminent living authors is, that the "first step in the process is the variation of the pupil, which seems, by a mechanism at the base of the iris, to increase the distance of the lens from the retina."* This is very vague; it is shewn by conclusive experiments that the simple contraction and expansion of the iris produces no effect on the focal adjustment;† and it is a mere conjecture that any of the organs connected with the iris, the ciliary body for instance, has, or can have, any influence in pulling the lens forward from the retina in any degree, much less through the considerable space requisite. We may, therefore, accept the *resumé* of a late French writer on Physics, as nearly expressing the opinion of the most candid authors upon this vexed subject: "Tout cela n'est pas très-satisfaisant, et il faut avouer que l'explication de la netteté de la vision à des distances si differentes est encore à trouver."

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* Quoted by MÜLLER.

† Nat. Phil. ii. 589.

‡ MÜLLER's Physiology, translated, p. 1143, 1144.

Such being the present phase of the question, the suggestion of a "possible explanation" yet unthought of, of the manner of the adjustment of the eye, may be received with indulgence, or at least proposed without presumption.

About three years ago, whilst lecturing on the subject of vision, I was struck with the circumstance, that the crystalline lens possesses not only a remarkable gradation of consistence or density from the centre towards the surface, and especially towards the edges, whereby, according to the common explanation, the spherical aberration of the rays of light is completely corrected; but likewise a complex and singular figure, which it is plain might alone produce the same effect by the modified curvature of the surfaces. Here, then, we appear to have two peculiarities of structure to attain one end; and it seems so natural, that the curves should be proper curves for destroying the aberration of sphericity, instead of the spherical curves which are used in our instruments only from our incapacity to form better ones,‡ that it occurred to me that the remarkable variations of density in the lens must be intended to answer another purpose.

This purpose I conceived might be the focal adjustment, and effected in the following way: The crystalline lens, for example, that of the ox, is composed of a nearly spherical nucleus of compact comparatively dense matter, of a hard pasty consistence, which gradually, yet rapidly, passes into the gelatinous envelope of a lenticular form, which has far less consistence, and less resistance to external pressure than the central spherule. It therefore occurred to me, that any *uniform* pressure applied to the lens, such as might be communicated by the external muscles of the eye to the entire eyeball, and propagated by hydrostatic pressure through the humours, would tend to make the exceedingly flattened ellipsoid of the eye approach in figure to the dense spheroidal nucleus; the obvious effect of which would be, without any change in the position of the lens, to increase its curvature, so as to render the rays from a near object more convergent.

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* Brewster in Art. Optics, Encyc. Brit. 7th Edit. p. 513.

† See Müller and Brewster.

‡ The forms of curvature of the crystalline lens are said to have been actually ascertained by M. Chossat to be ellipsoidal. It is a curious proof of the vagueness with which this subject has been treated, that, in the clear and able work of Professor Lloyd on Light and Vision, in one page, the form of the surfaces is insisted on as the means of producing distinct vision; and on another, the gradation of density from the centre to the side of the lens; whereas, it is certain, that if the compensation for spherical aberration due to the last cause be correct, the ellipsoidal form will be erroneous. Thus, as in many other cases, the argument for design has been made to prove too much. See Lloyd on Light and Vision, pp. 264-266, who refers to Chossat's paper, *Ann. de Chemie*, vol. x.

I proceeded, in April 1842, to endeavour to put my hypothesis to the proof, by subjecting the recent crystalline lens of a bullock to considerable hydrostatic pressure, in a suitable apparatus, and endeavouring to observe the change of focal distance produced, making it act as the object-glass of a microscopic arrangement; but, partly owing to the difficulty of suspending the lens in a secure yet free manner, partly from the unfavourable form of the glass vessel used for compression, partly from the small excess of refracting power of the lens above that of the water in which it was suspended, and partly from the essential indistinctness of the picture formed in the dead eye, and the consequent difficulty of determining its precise focal distance;—from all these causes my experiments failed in yielding a positive result:* and though I communicated my views soon after to Dr Alison, I postponed any farther consideration or publication of the subject, until I should be able to support the theory by decisive experiments. My attention has been wholly diverted since to other inquiries; and I see no prospect, at present, of resuming the experimental part, which, no doubt, would be worth pursuit, and though difficult, is not I think, hopeless. In the mean time, the subject of focal adjustment of the eye having been started at the late meeting of the British Association at York by Sir D. Brewster, it occurred to me to state verbally my notions; which having been thought worthy of attention, I have put them into this more definite and permanent shape.

In the absence of a direct proof in favour of my hypothesis (and this, it will be observed, no other theory possesses), I may be allowed to state one or two circumstantial evidences in its favour.

The first has been mentioned already, but is recapitulated for the sake of connection.

1. The crystalline lens possesses, on the common view, a twofold structure

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* It may be added, that the bullock's eye is perhaps one of the least favourable on which the experiment could be made. Owing to its very great convexity and thickness, it may be presumed that the action of compression above described will be much less visible than in a comparatively flat lens, such as that of man.

to produce a single end. I assume that each structural condition has a separate end; the variable curvature to correct the aberration, the variable density to alter the figure of it under pressure.

2. The attempt to view near objects distinctly is accompanied by a sensible muscular effort within the orbit. This is expressly stated, incidentally, by MÜLLER, in a passage already quoted; and has been admitted by every one whom I have questioned on the subject. From my own sensation, I have no doubt that it is a simultaneous effort of the four *recti* muscles drawing the eye back within its socket.* Such a retractive muscular action, fatal to the theory of elongation of the eyeball, is just what we require to communicate to the fluid humours of the eye, through the tough sclerotic coat in which they are bound, the hydrostatic pressure which will act simultaneously upon all points of the crystalline lens.

3. This theory is free from the unanswerable objections urged by Dr YOUNG and others, to all theories independent of that which ascribes the adjustment to change of figure in the lens: and it is free from the objection to Dr YOUNG's own theory, which presumes a structure existing in the lens itself, unproved, and, to say the least, improbable,—I mean its muscularity.

4. It is confirmed by the fact, that where the lens is reproduced after the operation for cataract, the power of adjustment is almost or totally lost; for, in that case, it cannot be supposed that the new lens is provided with the requisite gradation of coats for modifying its elasticity.

5. The diminution of the adjusting power of the eye in old age is well explained by the collapse and induration of the lens, to the detriment of its elastic properties.†

6. That the crystalline lens is actually possessed of variable elasticity in different directions is rendered highly probable by Sir DAVID BREWSTER's observations on its action on polarized light, in which tints are produced similar to those in compressed jellies, and in minerals possessing different axes of elasticity.

POSTSCRIPT TO THE PRECEDING PAPER.

When this paper was written, I had not seen CHOSSAT's paper on the Forms of the Refracting Surfaces of the Eye, which I have referred to. I have since read it,‡ and find a remarkable confirmation of the views I entertain.

It is very plain, that, were the gradations of the refrangibility of the coats of the crystalline intended to correct spherical aberration, this condition presumes the sphericity of the surfaces. If the surfaces have the curve of *no aberration*, for instance, an ellipsoid, with its longer axis parallel to the incident rays, any variation of density of the medium is not only useless but hurtful, producing a contrary error, and causing the central rays to converge too fast. On the contrary, if the variable density of the matter of the crystalline exist (which is an undoubted fact), and has been so arranged for a distinct purpose, the form of the surface of no aberration adapted to it will be a peculiar one, and, very probably, will be more convex towards the lateral parts than even the sphere, much more than the ellipsoid already mentioned, which is the curve of no aberration for a lens of uniform density.

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* I am aware that this is opposed to the experiment of Mr RAMSDEN and Sir E. HOME, which seems to shew a protrusion of the eyeball. Supposing it correct, that protrusion must be equally the result of muscular action producing pressure, due perhaps to the oblique muscles antagonising the *recti*; for it is difficult to see where else it can be sought.

† "In all animals the crystalline lens grows firmer with age." BREWSTER Edin. Encyc., Art. OPTICS, p. 475.

‡ In the Annales de Chimie, vol. x., published in 1819.

Having perceived this result, it was with no small satisfaction that I found, on examining M. CHOSSAT's paper, that whilst for the cornea (where the refraction is from air into the uniformly dense medium of the aqueous humour), the surface is that of an ellipsoid, with the longer axis in the direction of the incident rays, and which, therefore, destroys aberration by the appropriate curvature,—in the lens, the figure is that generated by an ellipse revolving round its *lesser* axis, and therefore possessing a contrary property to that ordinarily required for correcting aberration; the curvature being greater for the lateral than the central parts of the lens. This is surely an unanswerable proof, that the opinion maintained by, I believe, every modern writer on optics, without exception, namely, that the variable density of the lens is intended to correct *spherical* aberration, is a fallacy, since we find it combined with an appropriate figure for destroying *its* aberration, in which the peculiarities of spherical refraction are exaggerated.

The measures and drawings of M. CHOSSAT appear so minute and correct, as to leave no doubt of the fact of this antagonism to the common opinion.* Accordingly, neither by himself, nor by the few authors who have quoted this singular circumstance, has any explanation been given.

On our theory it is simple. The gradation of density has been provided for the mechanical purpose of varying the elasticity of the lens in different directions; and the form of its surfaces has been then determined so as to render the lens *aplanatic*.

---

* M. CHOSSAT's experiments were made on the lens of an ox; but DR ALLEN THOMSON tells me that, without knowing his results, he had arrived at a similar conclusion, as to the opposite kind of curvature in the *cornea* and in the *lens* of the human eye; the surface of the former lying without, and the latter within the surface of the osculating sphere.

EDINBURGH, 20*th December* 1844.

II.—*On the Modification of the Doubly Refracting and Physical Structure of Topaz, by Elastic Forces emanating from Minute Cavities.* *By* SIR DAVID BREWSTER, *K. H., D. C. L., F. R. S., and V. P. R. S., Edin.*

(Read 20th January 1845.)

WHILE examining, in polarised light, the form and structure of the numerous crystals which I had discovered in the fluid cavities of *Topaz*, my attention was particularly called to certain optical phenomena exhibited in other parts of the specimen. These phenomena, when first presented to me, were very indefinite in their character, and very imperfectly developed; but after a diligent examination of nearly 900 specimens of topaz, I succeeded in obtaining the most satisfactory exhibition of them under various forms, and in various degrees of intensity.

When an elastic force is propagated from a centre, in a soft and compressible medium, an increase of density is communicated to the surrounding mass,—of a temporary nature if the medium is a hard solid, like glass, but of a permanent nature if the medium is soft, and becomes indurated during the continuance of the compressing force. Both these effects may be exhibited experimentally, the first by a pressure upon glass, and the second by the action of an expanded bubble of air upon gum in a state advancing to induration.

The physical change thus produced in the transparent medium, whether it be temporary or permanent, may be exhibited to the eye in two ways, either by the property of the compressed parts in depolarising light, or in the unequal refraction of common light produced by a varying density, and consequently a varying refractive power. In the *first* of these cases, the depolarising action is displayed in the production of four quadrants of light, separated by the radii of a black rectangular cross, similar to the central portion, or the tints of the first order, in the uniaxal system of polarised rings; and, in the *second* case, the inequality of refractive density is shewn by the mirage of a luminous point, in the form of concentric circles surrounding the centre of force, each circle marking successive actions of the central force.

When the four luminous quadrants of depolarised light, shewn at A, B, C, D, in Plate, Fig. 1, first presented themselves to me, I had some difficulty in perceiving the seat of the force, by which I believed that they were produced. The centres, or intersections of the black cross, were either too deep beneath the surface of the topaz, or too much covered by fluid cavities, to be seen; but by removing the part of the crystal which contained these cavities, I succeeded in finding that, in every case there was a minute cavity in the centre of the luminous quadrants, or at the intersections of the arms of the black cross, from which the compressing force had emanated. One of these cavities is shewn at E., Fig. 2. It is of a quadrangular form, like the section of a rhomboidal prism, sometimes elongated, and sometimes of a slightly irregular shape. When perfectly regular, these cavities are between the 3000th and the 4000th of an inch in diameter. They are always dark, as if the elastic substance which they contained had collapsed into an opaque powder; and I have met with only one case in which there seemed to be a speck of light in the centre. The degree of compression to which the topaz has been subjected is measured by the polarised tint developed in the luminous quadrants. It varies from the faintest pale *blue* to the *white* of the first order. In one case I found the luminous quadrant of one cavity coinciding with a luminous quadrant of another cavity, and thus producing the sum of their separate tints. This effect is shewn in Fig. 3.

In the phenomenon now described, the elastic force has spent itself in the compression of the topaz. The cavity itself has remained entire, without any fissure by which a gas or a fluid could escape. I have discovered, however, other cavities, and these generally of a larger size, in which the sides have been rent by the elastic force; and fissures, from *one* to *six* in number, propagated to a small distance around them. These fissures have modified the doubly refracting structure produced by compression; but, what is very interesting, no solid matter has been left on the faces of fracture, such as that which is invariably deposited, when an ordinary cavity, containing one or both of the two new fluids, is exploded by heat. The form of some of the cavities which have suffered this disruption is shewn in Figs. 4, 5, and 6.

The influence of the compressing forces in altering the density, and consequently the refractive power of the topaz, is so distinctly seen in common light as to indicate the phenomena that are seen under polarised light. When the cavity is most distinctly perceived, it is surrounded with luminous and shaded circles, as shewn in Fig. 7; and traces of these are distinctly seen, as shewn in Fig. 8, when the specimen is examined in polarised light.

The cavities now described have obviously no resemblance whatever to those which I have described in previous papers as containing two new fluids. When any of the latter are either burst by heat, or exposed under high temperatures to the compressing forces of the fluids which they contain, they exhibit none of the phenomena peculiar to the former. The doubly refracting structure suffers no change; and when the cohesive forces of the crystal are overpowered, the faces of most eminent cleavage separate, and are covered with translucent crystalline particles, which the evaporated or discharged fluids leave behind.

The peculiar character of the pressure cavities, as we may call them, is still farther evinced by the nature of the specimens in which they occur. I have never found them accompanying the ordinary cavities with two fluids. The specimens which contain them have imbedded in them numerous crystals, differing little in their refractive power from topaz, and exhibiting in polarised light the most beautiful colours, varying with the thickness of the crystal, and diminishing in intensity as their axes approach to the plane of primitive polarisation.

It is impossible to review the preceding facts without arriving at the conclusion, that the topaz must have been in a soft and plastic state when it yielded to the compressing force which emanated from the cavities, and that a mineral body thus acted upon could not have been formed, according to the received theory, by the aggregation of molecules having the primitive form of the crystal.

In a letter to Sir Joseph Banks, printed in the Philosophical Transactions for 1805, I deduced, from my experiments on depolarisation, the existence of a new " species of crystallization, which is the effect of time alone, and which is produced by the slow action of corpuscular forces;" and I have remarked that " this kind of crystallization will probably be found to have had an extensive influence in those vast arrangements which must have attended the formation of our globe." These views have been confirmed by various new facts, wholly independent of each other;—by the existence of crystals imbedded in topaz, and having their axes in all possible directions, but especially by the nature and form of the strata of fluid cavities in that mineral. These strata cut at all inclinations the primary and secondary planes of the crystal. They are bent in the most capricious manner, forming planes of double curvature; and, what is also true of individual cavities stretching in every possible direction, they could never have been formed but when the topaz was in a soft and plastic state.

An objection to these views may be drawn from the fissures which proceed from the pressure cavities. The topaz must, doubtless, have been indurated when these fissures took place; but it is equally obvious that the depolarisation produced by compression must have previously existed, and it is probable that the fissures were produced after the crystal had been removed from its matrix, and when, from cleavage or otherwise, its cohesive forces had been diminished.

St Leonard's College, St Andrews,
January 16. 1845.

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III.—*On the Existence of Crystals with different primitive forms and physical properties in the Cavities of Minerals; with additional Observations on the New Fluids in which they occur.* By Sir David Brewster, *K.H., LL.D., F.R.S., and V.P.R.S., Edin.*

(Read Feb. 17, 1845.)

In 1823 and 1826 I communicated to the Society two papers on the nature and properties of two immiscible fluids, which I discovered, in contact with each other, in the cavities of topaz and other minerals. Although the facts contained in these papers were of so extraordinary a nature as to be received with scepticism by some, and with ridicule by others, yet I am not aware that, during the *twenty* years which have elapsed since their publication, any person has either repeated my observations, or advanced a single step in the same path of inquiry. In shewing to strangers some of the leading phenomena of the two new fluids, my attention has been frequently recalled to the subject; but it was not till last spring, when I discovered cavities in topaz filled with the most beautiful crystals of various form, that I was induced to undertake a new investigation of their nature and properties. In this investigation I have examined, with various magnifying powers, and both in common and polarised light, more than 900 specimens of topaz from Scotland, New Holland, and the Brazils; and I have had the good fortune to observe many new phenomena connected with mineralogy, chemistry, and physics, which, in addition to the interest which they may possess as scientific facts, promise to throw a strong light upon the existing theories of crystallization, and to bring before us some of those recondite operations which had been going on in the primitive rocks of our globe, before the commencement of vegetable or animal life.

1. *On the Form and Position of the Strata in which the Cavities lie.*

The cavities which contain the two new fluids, and their accompanying crystals, sometimes occur single, and in groups more or less numerous; but, in general, they exist in millions, occupying extensive strata, which affect the transparency of the mineral, and render it unfit for the use of the jeweller, or even for the cabinet of the collector, who has not learned that it is in the deviations from her ordinary laws that Nature often discloses her deepest mysteries.

Although the strata of cavities sometimes occur, as in artificial salts, in planes parallel to the primary or secondary forms of the crystal, yet they occupy every *possible position* in reference to these planes; and we, therefore, cannnot account for them by supposing that certain spaces have been left in the crystal, without the primitive molecules which ought to have been there deposited. The strata of cavities, too, have every possible curvature. From a plane surface they pass into a curved one, sometimes of variable curvature, and sometimes of contrary flexure, cutting and intersecting each other in the most capricious manner.

In the shape of the strata the same irregularity presents itself; their outline is sometimes rectilineal, sometimes curved, and sometimes singularly irregular In some specimens the whole crystal is intersected with the strata; and it is extremely probable, though it is impossible to determine the fact, that in every specimen some edge or angle of the stratum touches the surface.

The succession of the cavities in composing the stratum, and their form in relation to the character of the stratum, present interesting phenomena. I have found specimens in which the cavities lie in concentric arches, and have their sides concentric, and, as it were, a portion of the same arches, as if they had been formed under the influence of a rotatory force. In other cases they occupy parallel lines, and are sometimes so equidistant that they might be advantageously used as micrometers for microscopes. In one remarkable specimen they radiate from a centre, each radiation having a character of its own. One radiation will sometimes throw off a diverging branch, while two or more radiations will converge and then diverge again, subsequently uniting themselves into a single radiation.

When different strata of cavities lie parallel to each other in the specimen, which they sometimes do, to the number of *four* or *five*, each stratum has generally a distinct character; flat and exceedingly thin cavities occupying one stratum, very deep cavities occupying another, minute cavities which the highest magnifying powers can scarcely resolve occupying a third, while a fourth consists of the most irregular and indescribable forms.

When the forms of individual cavities are related to that of the stratum which contains them, they, of course, cut at all angles the primary and secondary planes of crystallization; and the same is true of insulated cavities of great length, which are sometimes turned, and twisted, and bent, in the most capricious manner. It is impossible to read these details, and still more so to study the phenomena themselves, without being driven to the conclusion, that the strata of cavities must have been formed under the influence of forces propagated through a soft and plastic mass, and carrying along with them gases and vapours which came to a position of rest previous to the regular crystallization of the topaz. This conclusion, which I have been led to draw, in another paper, from a series of entirely different facts, will be still further confirmed by the phenomena of imbedded crystals, to which I shall have to refer in another section.

17 MR 58

Fig. 1.

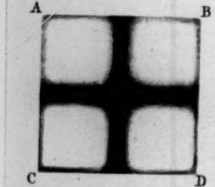


Fig. 2

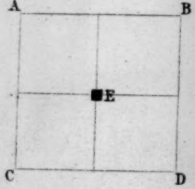


Fig. 9.

Fig. 10.

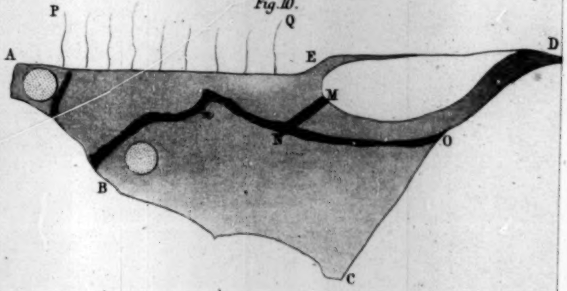


Fig. 3

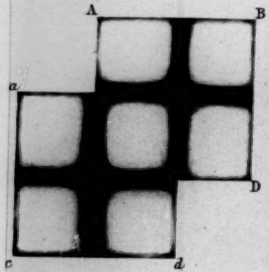


Fig. 4.

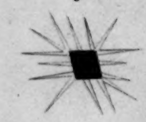


Fig. 11.

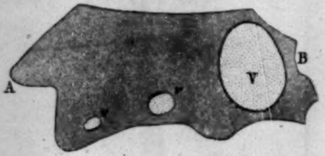


Fig. 12.

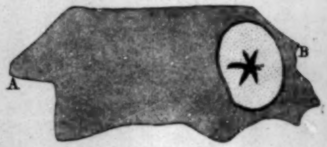


Fig. 13.

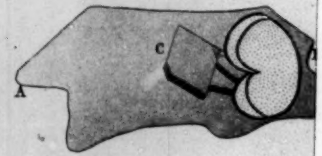
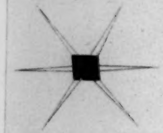


Fig. 5.



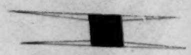


Fig. 14.

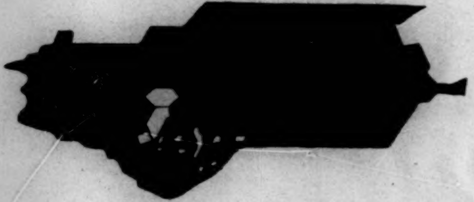


Fig. 15.

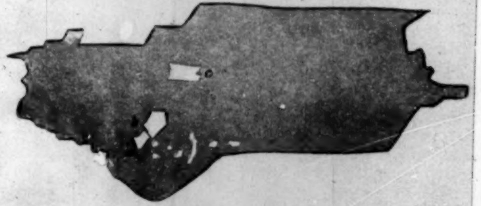


Fig. 6.

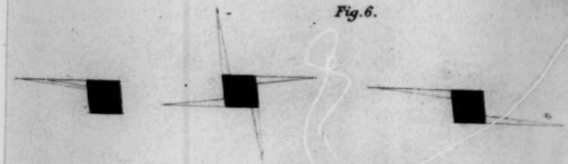


Fig. 7.

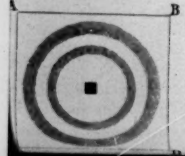


Fig. 8.

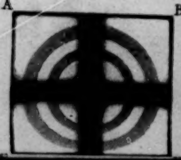


Fig. 16.

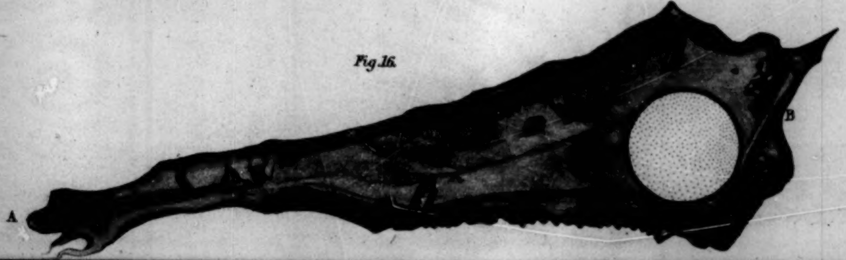


PLATE 1 Royal Soc. trans. Edin. Vol. XI p.

2. *Additional Observations on the Nature and Properties of the two New Fluids.*

In re-examining the phenomena exhibited by the two new fluids, I have found no occasion to modify or to correct any of the results contained in my former papers. In the cavities which appear to contain only one fluid, namely, the dense fluid, I have sometimes found a very small quantity of the volatile fluid, which, with a slight rise of temperature, passes into vapour, and prevents the apparent vacuity from disappearing by the application of a strong heat. When there is no volatile fluid present in such cavities, the vacuity is a real one, and disappears entirely by the application of such a heat. If the heat is not instantly withdrawn on the disappearance of the vacuity, the crystal never fails to burst with great violence.

In some specimens of Brazil topaz I have found cavities with two fluids, and without any vacuity in the volatile fluid at the ordinary temperature of an apartment. In such cases I have generally produced a vacuity by the application of ice. Had heat been applied, the crystals would have burst, as there were no empty spaces into which the fluids could expand.

When the cavities are flat, and have their faces perpendicular to the axis of the crystal, or parallel to the planes of *easy cleavage*, the application of heat does not burst the crystal, but produces a very remarkable phenomenon. The cavity opens at its weakest point, and the fluid passes by starts, through a succession of resting places, to another part of the crystal where it finds the readiest exit. The fluid penetrates, as it were, the solid gem, and the laminæ which it has forced asunder in its passage, again close into optical if not into mechanical contact. If the heat is withdrawn when the first minute drop has passed, the laminæ unite, and we can discharge the rest of the fluid whenever we please till the cavity is exhausted. This phenomenon is represented in Plate, Fig. 9, where A B C D is a shallow cavity in a plate of topaz M N, and E F another cavity, which has been emptied of its fluid contents by reaching the surface at N, where it had been broken through. Upon looking at the cavity A B C D when slightly heated, I observed dark portions of fluid rushing from its sharp termination at D through the cavity at *a*, and then reappearing at *b* and *c*, and then passing into the empty cavity E F. The small lakes, as we may call them, at *a*, *b*, and *c*, disappeared entirely when the discharged portions of fluid had passed, and reappeared with a change of form and size when the operation was repeated.

In a specimen of topaz possessed by Major Playfair, and seen by many individuals, a white ball passed from one cavity to the edge of the specimen, as if projected from a mortar; but by the application of too strong a heat it was shattered in pieces.

In my first paper of 1823,* I have described and figured a phenomenon of an analogous kind; but as it appeared unexpectedly, and was instantly followed by the explosion of the crystal, I could neither observe it accurately, nor confirm what I did observe, by a repetition of the experiment. I have, therefore, some satisfaction in describing a similar phenomenon, seen frequently, and under more favourable circumstances, not only from its intrinsic interest, but because a distinguished philosopher had treated with an air of incredibility an observation which I had made of a similar kind. There can be no higher testimony to the novelty and importance of a scientific fact, than when a competent judge raises it to the supernatural.

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* *Edinburgh Transactions*, vol. x. p. 11, Plate I. Fig. 5, 6.

I come now to describe a property of the dense fluid, so new and remarkable that it cannot fail to excite the attention of chemists. This fluid occupies the whole of a large cavity A B C D E, Fig. 10, with the exception of a bubble at A, which must be either a vacuum, as it is in all cavities containing only this fluid, or a bubble of the expansible fluid, or the vapour of the dense fluid, or some gaseous body. It cannot be a vacuum; because it expands with heat, in place of being filled up by the expansion of the fluid. It cannot be the expansible fluid; because cold would contract it, and produce a vacuity. It cannot be the vapour of the expansible fluid; because there is no expansible fluid to throw it off, and it has not the optical properties of its vapour. It cannot be the vapour of the fluid in the cavity; for it does not disappear by the application of cold, and does not become a vacuity, which fills up by the expansion of the fluid. It is, therefore, an independent gas, which exhibits the following phenomena.

When heat is applied, the bubble A expands, not by the degradation of its circular margin passing into vapour, as in the vapour cavities described in a former paper, but by the rapid enlargement of its area. When it attains a certain size, it throws off a secondary bubble B, which passes over a sort of ridge or weir *m n o*, in the bottom of the cavity, and settles at B. If the heat is continued, these two bubbles increase in size; but it was instantly withdrawn when B had begun to swell. As the topaz began to cool, both the bubbles A and B quickly contracted. The primary bubble A returned gradually to its original condition, and B, when reduced to a single speck, would have disappeared, had the cooling not been stopped. This speck swelled again by the application of heat, and so did the bubble A. When the speck at B was allowed to vanish, which it did on the spot which the bubble occupied, the fresh application of heat did not revive it at that spot, but merely expanded the primary bubble A, which again threw off a secondary bubble B, which exhibited by heat and cold the same phenomena as before. These experiments I repeated many times with the same result. It will naturally be asked, what was the condition of the fluid itself which has the property of expanding by heat; and what became of it while a part of the space which it occupied was appropriated by the bubble B, and the addition to the bubble A? An accidental circumstance enables me to answer this question, which would have been otherwise a very perplexing one. Having applied too strong a heat to the specimen, the bubble A threw off beside B two or three smaller ones, which moved along the upper edge A E. My attention having been thus directed to this part of the specimen, I was surprised to observe a great number of capillary lines or pipes P Q, rising from the edge A E of the cavity, and into which the fluid was forcing itself, oscillating in these minute tubes like the mercury in a barometer, and sometimes splitting the laminæ between them. The force of cohesion, thus overcome by the expansive efforts of the fluid, predominated over the capillary attraction of the tubes and surfaces, and pressed back all the fluid into the cavity, when the body of fluid had contracted in cooling.

If we now consider the body which occupies the vacuity A as a gas, and, consequently, the other bubble B as the same, it follows, that the whole of the gas in B was absorbed by the fluid while cooling, and again given out by an increase of temperature. The gas, when in the act of being discharged, took its course to the locality of the speck at B, and to the bubble A; but to the bubble A alone when the speck had disappeared.

Upon repeating these observations the cavity burst; and I have now before me its two halves, forming its upper and its under surface. The portion of the cavity at A has the same depth as the portion below *m n o*, all the rest of the cavity being much shallower. There was a fine doubly refracting crystal at M N, which polarised the blue of the second order; and its outline is still left on the cavity. There was a sort of crystalline powder disseminated round M N to a considerable distance, and the roof of the bubble B, when the roof of the cavity was entire, was always mottled with this powder.

In a former paper, I have distinguished vapour cavities from common cavities, by the manner in which the vacuity in the expansible fluid disappears. In the one case, the vacuity gradually enlarges by the degradation, as it were, of its margin, as the fluid passes into vapour; in the other, the vacuity gradually diminishes till it disappears. I have since found cavities of an intermediate character, in which the vacuity, on the first application of heat, diminishes, and then, when it has contracted to a certain size, it begins to expand; and its margin becoming thinner and thinner, it finally passes into vapour.

3. *On the Form and Position of Crystals in the Cavities of Topaz.*

In a former paper I have described a moveable group of crystals of carbonate of lime, which I discovered in a cavity in quartz from Quebec, containing a fluid with the properties of water. The crystals to which I am about to call attention, are of a very different kind, and possess a very different kind of interest.

The crystals which occupy the fluid cavities of topaz are either fixed or moveable. Some of the fixed crystals are often beautifully crystallized. They have their axes of double refraction coincident with those of the crystal, and, as I have ascertained by the examination of exploded cavities, they actually form part of the solid topaz, though they exist in the fluid cavity. One or two of these are shewn in Fig. 4, Plate XIX., of my paper of 1826,* and they may be distinguished by their attachment to the sides of the cavity. In the same figure, as well as in Figs. 10, 13, 20, and 21 of my Paper of 1823,† I have drawn others which I then believed to be fixed, but which I have no doubt are moveable, and produced from one or other of the new fluids.

In re-examining my specimens of *topaz*, I have been surprised at the great number of cavities which contain crystals. In some there are only one ; in very many there are two, three, and four ; and in a great number of specimens the cavity is so crammed with them, like a purse full of money, that the circular vacuity has not room to take its natural shape, and often can scarcely be recognised, in its broken-down condition, among the jostling crystals.

The crystals of which I am treating are sometimes found in the volatile, and sometimes in the dense fluid, but chiefly in the latter. They are often found in an amorphous state in the narrow necks and narrow extremities of cavities, positions in which they remain fixed while they continue solid ; and sometimes regularly formed crystals remain fixed between the prismatic edges of cavities, in consequence of having either fallen into that position, or of having been formed there.

The crystals in topaz cavities are, in general, beautifully crystallized, and have a great variety of forms. I have observed the following :—

1. The Tetrahedron.
2. The Cube.
3. The Cube, truncated on its edges and angles.
4. The Rhombohedron.
5. The Prism, with plain and pyramidal summits.
6. The Flat Octohedron, truncated on its edges and angles.
7. Rhomboidal Plates.
8. Hexagonal Plates.
9. Long rectangular Plates.

Besides these, there are amorphous crystals and crystallized masses of various characters.

4. *On the Physical Properties of the Crystals in Topaz Cavities.*

Although it would be desirable to submit these crystals, as well as the fluids which contain them, to chemical analysis, yet the task is too difficult to be accomplished in the present state of chemical science. I must, therefore, limit my observations to such of the physical properties of these crystals as can be rendered visible to the eye.

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* *Edinburgh Transactions*, vol. x.

† Ibid., Plates I. and II.

When I first applied heat to the crystals under consideration, I employed a very fine specimen, with large and numerous crystallized cavities, of a prismatical form, containing both the new fluids. In this specimen, there were seven cavities unlike all the rest, and each of them containing a single crystal, and apparently but one fluid, namely, the dense one. The cavities were exceedingly flat, and irregular in their shape, and very unlike one another. Upon applying the heat of only a lighted paper match beneath the plate of glass on which the specimen lay, I was surprised to see the crystals gradually lose their angles, and then slowly melt, till not a trace of them was visible. In this state, one of the cavities had the appearance shewn in Fig. 11, where V was the vacuity, and *v*, *v′*, other two bubbles, one of which *v* soon joined the principal one V. In all the other six cavities, the crystals were speedily reproduced, always at the point where they disappeared, provided a small speck remained unmelted; but otherwise in different parts of the cavity. In the cavity A B, however, Fig. 11, the crystal was very long in appearing. In the course of an hour, however, a fasciculus of minute crystals appeared in the centre of the vacuity, as in Fig. 12, and to them the principal crystal attached itself, as in Fig. 13, which exhibits a perfect rhomboidal plate, truncated on its obtuse angles. The elliptical vacuity was pressed into the shape of a heart; and, by the application of ice, I succeeded in precipitating the vapour of the expansible fluid, which existed in a very minute quantity in all the seven cavities. The expansible fluid is shewn between the two heart-shaped outlines in the figure, and I repeatedly threw it into vapour, and reduced that vapour to a fluid state. The phenomenon now described, of the melting of the crystals, and their subsequent re-crystallization, I have shewn to various persons; and it is very remarkable that they generally reappear in this specimen of the same form, though with considerable modifications.

Upon applying heat to other cavities, containing several crystals, I obtained very different results. Some of them melted easily, others with greater difficulty; and some were not in the slightest degree affected by the most powerful heat I could apply. When the crystals melted easily, they were as quickly reproduced; sometimes reappearing more perfectly formed than before, but frequently running into amorphous and granular crystallizations.

In some specimens of topaz, all the crystals in the cavities refuse to melt with heat, and seem not to suffer the slightest change in their form. Hence we are entitled to conclude, that the crystals possessing such different properties must be different substances; and this conclusion is amply confirmed by an examination of their optical properties.

In making this examination, I used a polarising microscope, so constructed that the plane, passing through the optical axis of the topaz, could be readily placed either parallel or perpendicular to the plane of primitive polarisation. In this case, the field of the microscope is wholly obscure, in so far as the depolarising action of the plate of topaz is concerned; but if there is any crystal in the topaz, either imbedded in its mass, or included in its cavities, that crystal will exhibit its doubly refracting structure, if it has any, by its depolarising action. It may, indeed, happen,—and it does happen,—that the plane passing through their optical axes coincides, either accurately, or so nearly, with that of the topaz, that its depolarising action is a minimum; but an experienced observer will have no difficulty in distinguishing this want of depolarisation by position, from the want of it by structure.

When the specimen of topaz is rich in cavities full of crystals, the display of luminous and coloured crystalline forms in the dark field of the microscope, indicating, too, the imprisonment of fluids, and the condensation of gases before vegetable or animal life had visited our primeval globe, was as interesting to the imagination and the judgment as it was beautiful to the eye. Having had the privilege of being the first to see it, I felt the full influence of the sight; and I have again and again contemplated it with renewed wonder and delight. When the cavities are so numerous as to mock calculation, and so infinitely small as to yield no visible outline to the highest powers, the bright twinkle of a crystalline atom within them reveals to us their nature as well as their contents.

In the examination of the individual crystals, many interesting facts present themselves to our notice. The crystals of the tessular class, which are modifications of the cube, are very numerous, and have no action upon polarised light. Many of them melt easily, while others refuse to yield to the action of heat; and hence, there must be two different substances in the cavities which assume the same shape. In like manner, some of the doubly refracting crystals melt readily, others with very great difficulty, and others not at all; so that there must be *three* different substances, which belong to the classes of forms that give double refraction; a conclusion which is confirmed by the different secondary forms which I have already enumerated.

I have seldom found any crystals in these cavities which depolarise white light, or the highest order of colours. I have found some that depolarise *four* orders of colours; and when the crystal which does this is a flat hexagonal plate, it is highly interesting to see it pass through all the tints which these orders include, while slowly melting, and again reproducing them during its re-crystallization.

In a cavity which was so placed as to be entirely black from the total reflection of the light which fell upon it, I observed three *white* openings, *a*, *b*, *c*, of a crystalline form, see Fig. 14. These appeared to be fixed crystals, or rather parts of the topaz, surrounded by a cavity. I found, however, that the hexagonal one

C depolarised white light, while the rest had no action upon polarised light. Upon applying heat, the crystal *c* melted, and took up a position at *c* Fig. 15, in a narrower part of the cavity, where it remains of an irregular form, having been repeatedly melted and re-crystallized. Upon turning the cavity into a position where it became transparent, I found that there was no fluid whatever in the cavity; so that we have here an example of a crystal melting and re-crystallizing without having been dissolved in one of the fluids. From the irregular state of the laminæ close to this cavity, there is every appearance of the fluids having escaped from one of its extremities.

In the course of these observations, I observed a phenomenon, produced by heat, of the most novel and surprising kind, and one which I feel myself utterly unable to explain. It presented itself when I was studying the very interesting collection of crystals in the cavity A B, Fig. 16. This cavity is filled with the dense fluid, in which there is a vacuity V: the fluid swells to such a degree with heat as to diminish very perceptibly the size of this vacuity; and as I can find no trace of any portion of the volatile fluid, I have no doubt that this vacuity would disappear by an increased degree of heat. The fear, however, of bursting so rare and interesting a cavity, has prevented me from making this experiment. The cavity contains a great number of crystals of different forms, not one of which melts with heat, and almost all of which possess double refraction. When I first submitted this cavity to the microscope, there were *five* small crystals lying between D and the vacuity V; one a flat prism, another a hexagonal plate, a third amorphous, and a fourth and fifth two irregular halves of a hexagon. Upon the first application of heat, one or two of these crystals leapt from their resting place, and darted to the opposite side of the cavity. In a few seconds, the others quitted their places one after another, performing the most rapid and extraordinary rotations. One crystal joined another, and, at last, four of them thus united revolved with such rapidity as completely to efface their respective shapes. They then separated on the withdrawal of the heat, and took the position which their gravity assigned them. On another occasion, a long flat prism performed the same rotation round its middle point; and I have repeated the experiment so often, in shewing it to others, that the small crystals have been driven between the inclined edges of the cavity, from which I cannot extricate them. I have succeeded, however, in conducting a fine octohedral crystal, truncated on its edges and angles, into the arena at D, where I have just seen it perform its rotation, as indicated by the concentric circles on the right hand of D.

In seeking for the cause of so extraordinary a phenomenon, we are reminded of the rotations of camphor and other volatile substances; but, in this case, no gas or matter of any kind could be thrown off without becoming visible in the fluid. The pyro-electricity of topaz next suggests itself as a moving power; but though it might produce attractions and repulsions, we cannot see how it could turn a crystal upon its axis. The experiments of Libri and Fresnel, on the repulsions which heated bodies exert upon each other at sensible distances, afford us as little aid. They may enable us to account for the mere displacement of the crystals by the application of heat, or for their sudden start from their places of rest, but they do not supply us with a force fitted to give and to sustain a rapid rotatory movement.

I have already had occasion to state, that the cavities often burst when too much heat is applied to the specimen. This generally takes place by a separation of the laminæ, which fly off in splinters; but when the burst cavity is large and insulated, a piece of the solid crystal is scooped out on its weakest side. Sometimes a great number of cavities explode at the same time, and when they are small, or exist in a part of the crystal where there are no large ones, the explosive force is not strong enough to separate the laminæ. The fluid is merely driven between the laminæ to a small distance around the cavity, and shews itself as a dark brown powdery matter, encircling the cavity as the burr of a comet does its nucleus. When the cohesion of the laminæ is great, it resists the explosive force over a large cavity, and the contents of the cavity are thrown to a considerable distance around it, and remains between the laminæ, either as a sort of powder, or as a congeries of minute crystals, which are sometimes large enough to shew their depolarising action. When the laminæ separate, we find this crystalline matter either fluid or indurated; exhibiting, when fluid, the extraordinary properties described in my former papers. If we breathe upon the indurated matter it becomes fluid, re-crystallizes in new spiculæ and crystals; and, on several occasions, I have found fine examples of circular crystallization.

After the explosion of cavities containing only the dense fluid, I have been surprised to find, and that in large cavities, that no trace of matter was left upon the sides of the cavity or around it. Whether this arose, as the fact seems to indicate, from the dense fluid being a condensed gas, or from some other cause, it will require new experiments to determine.

In a very remarkable specimen, in which the cleavage plane passed through a great number of large flat cavities, the brown matter has been lodged near to the edges of each cavity, and marks them them out even to the unassisted eye. These cavities were filled almost solely with the volatile fluid; and since the faces of the cavities are corroded as if by the action of a solvent, developing crystalline forms, there is reason to think that the fluid has exercised this action, and that the phenomenon is analogous to that external action, on the faces of hundreds of Brazil topazes in my possession, which I have described in the Cambridge Transactions,* and the singular optical figure formed by which, I have represented in a late volume of the Transactions of this Society.†

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* *Cambridge Transactions*, vol. ii. Plate i. fig. 15.

† *Edinburgh Transactions*, vol. xiv. Plate x., fig. 1, 2.

The only chemical experiment on the contents of these cavities, which I have had occasion recently to make, is perhaps worth reporting. One angle of a cavity was blown off by its explosion, and though the fluids escaped, a pretty large prismatic crystal remained within the cavity. I introduced *water* and *alcohol* successively into the cavity, and raised them to a considerable heat; but they had no effect in dissolving the crystal.

5. *On Solid Crystals and Crystalline Masses imbedded in Topaz.*

Among the new phenomena which this section embraces, there is at least one intimately connected with the subject of the fluid cavities. How far the other phenomena may have any such connexion, it remains to be seen.

The imbedded crystals to which I refer, presented themselves to me while the specimens which contain them were exposed to polarised light. Mineralogists have been long familiar with the beautiful crystals of Titanium, imbedded in quartz, and I have found the same mineral imbedded under still more interesting circumstances in the Brazilian amethysts.

In topaz, however, the imbedded crystals have never been noticed, and I have fortunately obtained specimens, in which they are displayed with singular beauty. Their axes of double refraction are not coincident with those of the topaz; and hence they are seen in the obscure field of the microscope splendent with all the colours of polarised light. These crystals are equally transparent with the topaz, with a few slight exceptions They sometimes polarise five or six orders of colours; and, in general, they have very beautiful crystalline forms, which can be seen by the microscope in common light. In some cases, they are mere crystalline masses, often of a reniform shape, but still with regular axes of double refraction.

In some specimens of Brazil topaz, the crystals occur in branches or groups of singular beauty, consisting of prisms and hexagonal plates, connected apparently by filaments of some opaque matter.

I have, occasionally, met with another interesting variety of them, which *have no visible outline* by common light, and which could never have been detected but by the polarising microscope. In one of these cases, the crystalline mass, which is nearly spherical, lies in a crowded group of small fluid cavities, none of which enters its mass; a complete proof that the cavities were formed in the soft mass of topaz, when it encircled the indurated crystal.

Along with these interesting phenomena, another occasionally occurs, which may still require a farther examination. I have observed *apparent* doubly refracting crystals, which differ in some essential points from those which have been described. They depolarise a uniform, or nearly a uniform tint, notwithstanding the different thicknesses through which the polarised light passes; and that tint is less brilliant than in the real imbedded crystals. I conceive, therefore, that they are crystallized cavities, having their inner surfaces coated with a doubly refracting crust. This is, in itself, a very natural supposition, seeing that the fluid may have discharged its gaseous portion, and left behind it the matters which it held in solution. The cavities, however, of this kind, which I have described in a former paper, have no depolarising action; and I find that those now under consideration have regular axes of double refraction. Hence, the matter which covers them must be a regular crystalline shell, with optical and crystallographic axes—a phenomenon which has no parallel in mineralogy.

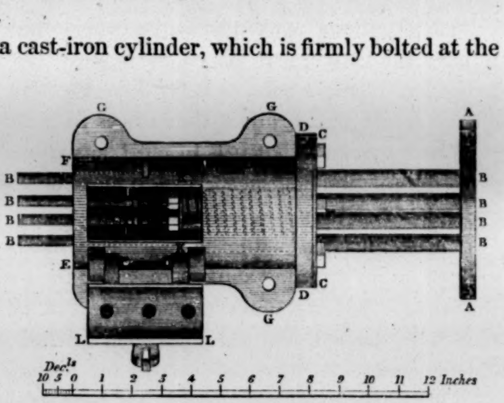
ST LEONARD'S COLLEGE, ST ANDREWS,
February 15. 1845.

IV.—*Account of Experiments upon the Force of the Waves of the Atlantic and German Oceans.* By Thomas Stevenson, Civil-Engineer, Edinburgh. Communicated by David Stevenson, Esq.

In forming designs of marine works, the engineer has always a difficulty in estimating the force of the waves with which he has to contend. The information on such a matter, which is derived from local informants, who, although intelligent in the departments of trade which they follow, are, nevertheless, more or less prejudiced from being constantly on the spot, is not satisfactory; and it has, therefore, often occurred to me that it would be most desirable if the engineer could be enabled, to some extent at least, to disregard the prejudiced statements of others, and the vague impressions left by them on his own mind, and really to ascertain, by direct experiment, what force, expressed in pounds per square foot, the sea actually exerts upon the shores where his buildings are proposed to be erected.

Notwithstanding the want of all direct experiments* on this subject, and the somewhat unpromising nature of such an enquiry, I was, nevertheless, induced to attempt the construction of an instrument to effect the desired end; and after several fruitless devices had been put to the test, I at length succeeded in forming one whose indications I hope to be able to shew are trustworthy. Before considering the results obtained, however, I shall explain the construction of this simple self-registering instrument.

The letters D E F D represent a cast-iron cylinder, which is firmly bolted at the projecting flanges G to the rock where the experiments are wanted. This cylinder has a flange at D D. L L is a door, which is opened when the observation is to be read off. A A is of iron, and forms a circular plate or disc, on which the sea impinges. Fastened to the disc are four guide-rods B B B B. These rods pass through a circular plate C C (which is screwed down to the flange D D), and also through holes in the bottom E F. Within the



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* Sir S. Brown has indeed stated, that at Brighton he found the impetus of the waves during heavy gales was "equal to 80 lb. to a foot upon a cylindrical column of 12 inches diameter." The hydrostatical pressure of a wave only 1¼ foot high is equal to 80 lb. upon a square foot.

cylinder there is attached to the plate C C a powerful steel spring, to the other or free end of which is fastened the small circular plate K K, which again is secured to the guide-rods B B B B. There are also rings of leather T T, that slide on the guide-rods, and serve as indices for registering how far the rods are pushed through the holes in the bottom; or, in other words, how much the spring has been drawn out or lengthened by the force of the sea acting upon the plate or disc A A. The object of having four leathern rings, where one might have answered the purpose, was merely that they might serve as a check upon each other; and so perfectly did they answer the purpose intended, that in every instance they were found equidistant from the bottom of the cylinder; proving thereby, that, after the recoil of the spring, they had all kept their places. The guide-rods are graduated, so as to enable the observer to note exactly the quantity that the spring has yielded.*

This instrument, which may, perhaps, be not improperly termed a *Marine Dynamometer*, is, therefore, a self-registering apparatus which indicates the maximum force of the waves. In the graduation of the instrument, the power of the spring is ascertained by carefully loading the disc with weights, so that when the quantity that the spring has yielded by the action of the sea is known, the pressure due to the area of the disc exposed is known also. The discs employed were from 3 to 9 inches diameter, but generally 6 inches, and the powers of the springs varied from about 10 lb. to about 50 lb. for every $\frac{1}{8}$ inch of elongation. Their respective effects were afterwards reduced to a value per square foot. The instrument was generally placed so as to be immersed at about three-fourths tide, and in such situations as would afford a considerable depth of water. It is not desirable to have the instrument placed at a much lower level, as it has not unfrequently happened during a gale, that for days together no one could approach it to read off the result and readjust the indices to zero. It must, however, at the same time be remarked, that it is in most situations almost impossible to receive the force unimpaired, as the waves are more or less broken by hidden rocks or shoal ground before they reach the instrument.

In connection with the apparatus above described, a graduated pole was erected on an outlying sunken rock, for the purpose of ascertaining the height of the waves; but the observations were not of so satisfactory a nature as could have been desired, and the poles soon worked loose from their attachments, and disappeared.

With the instrument which has been explained, I entered upon the following train of observations:—

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* It has been suggested to me, that the indications of the instrument might be made through the medium of a flexible wire or chain at a considerable distance from the instrument, and thus the impulse of every wave might be observed.

In 1842 several observations were made on the waves of the Irish Sea at the island of Little Ross, lying off the Bay of Kirkcudbright. Since April 1843 till now, continued observations have been made on the Atlantic at the Skerryvore and neighbouring rocks, lying off the island of Tyree, Argyllshire. And in 1844 a series of similar observations was begun on the German Ocean at the Bell Rock. It will be seen, that in selecting these localities a varied exposure has been embraced, comprising the comparatively sheltered Irish Sea, the more exposed eastern shore of Scotland, and the wild rocks of Skerryvore, which are open to the full fury of the Atlantic, the far distant shores of North America being the nearest land on the west.

Referring for more full information to the tables of experiments which are given at the end of this paper, it will be sufficient in this place to state generally the following, as the results obtained.

In the *Atlantic Ocean*, according to the observations made at the Skerryvore rocks, the average of results for five of the *summer* months during the years 1843 and 1844, is 611 lb. per square foot. The average results for six of the *winter* months (1843 and 1844), is 2086 lb. per square foot, or thrice as great as in the summer months.

The *Greatest result* yet obtained at Skerryvore was during the heavy westerly gale of 29th March 1845, when a pressure of 6083 lb. per square foot was registered. The next highest is 5323 lb.

In the *German Ocean*, according to the observations made at the Bell Rock, the greatest result yet obtained is 3013 lb. per square foot.

It thus appears, that the greatest effect of the sea, which has been observed, is that of the Atlantic at Skerryvore, which is nearly equal to three tons per square foot.

These experiments, amounting to 267 in number,* and on the Atlantic alone extending over 23 months continuously, are not intended to prove any thing farther than the simple fact, that the sea has been known to exert a force equivalent to a pressure of three tons per square foot, however much more. Now, when we consider that the hydrostatic pressure due to a wave of 20 feet high, is no more than about half a ton on a square foot, we see how much of their force the waves owe to their velocity. There can be no doubt, however, that results higher than this will be obtained. Were a train of observations made at various points of the coast, the result would not only be highly useful in practice, as they would by reference to existing marine works shew what sizes of stones and proportions of piers were able to resist seas of a given force; but they would form an interesting collection of information with regard to the relative forces of the waves in our contracted bays and estuaries, as compared with those observed in the ocean; and would thus supply the want which, as already stated, all engineers labour under, to a greater or less degree, in designing marine works.

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* It was not thought necessary to give all the observations in the table appended to this paper.

It is proper, however, to observe, that there may be some objection to referring the action of the sea to a statical value. Although the instrument might perhaps be made capable of giving a dynamical result, it was considered unnecessary, in these preliminary experiments, to do any thing more than represent the maximum pressure registered by the spring, because the effects of the waves may, from supposing them to have continuity of action, be perhaps regarded as similar to a statical pressure, rather than to the impact of a hard body.* The near coincidence, or indeed almost perfect agreement of the results of the experiments made with different instruments, goes far to shew that the waves act in very much the same manner as a pressure, although both pressure and impact must obviously enter into their effect. In the experiments, begun February 1844, and given at the end of the paper, the three instruments had not only different areas of discs, but very different powers of springs, and yet the results were almost identical. Now, the same force, supposing the waves to act like the impact of a hard body, would, in the Marine Dynamometer, assume very different statical values, according to the spaces in which that force was expended or developed; so that with the same force of impact, the indication of a weak spring would be less than that of a stronger.

In future experiments it may be interesting, however, to test the springs dynamically, by means of the impact of a heavy body dropped from a given height upon the plate or disc of the instrument. In some experiments lately made in this way, by dropping a cannon-ball upon the disc, it appeared, that, within the limits of the experiments, there was for each individual spring a ratio between the value registered by the leathern index and the calculated momentum of the impinging body. These ratios were, of course, found to vary in springs of different power, and to be constant only for springs of the same power. Did the waves, therefore, act by a sudden finite impact, like the cannon-ball employed in this instance, we could scarcely have found such harmony between the results of instruments with different springs, as the experiments alluded to afford. At the same time, the result cannot, perhaps, be in strictness considered correct; but, from the elongation of the spring being very small, the results may be regarded as practically correct,—the more so when we find so remarkable a coincidence of results as that alluded to.

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* With reference to the continuous action of water, I may notice the effects produced by the failure of Beith's Dam, a reservoir situated upon the high grounds near Cartsdyke, immediately east of Greenock. This dam had a head of 20 feet of water, and gave way on the night of the 21st November 1835, when the water, after breaking down another reservoir below it, rushed through the streets of Cartsdyke, causing the melancholy loss of no fewer than 41 lives. This continuous flow of water carried away many houses; and, among other instances of its power, it is recorded that a "mass of rock about 16 tons weight was borne along by the torrent to a distance of 30 or 40 yards." This case, then, which almost equals the records of the fury of the sea, shews the effects which continuous action may produce.

I shall now contrast the indications of the Marine Dynamometer by stating a few facts regarding the ascertained effect of the waves in the elevation of spray, and in the transportation of heavy masses of rock. This is more especially important, as to some, the results indicated by the instrument have appeared greater than they could have expected; and it has even been supposed that, were they correct, the stones which constitute our marine works would be scattered. Before passing from this point, it may be well to observe that the stones composing sea-works, are not only wedged and compacted together, but they derive from the superincumbent courses, (independently of the support afforded by the *backing*), a pressure so great as to cause an amount of friction which is in most cases greatly more than sufficient to preserve them in their places.

But to return to the facts of the ascertained effects of the waves, it may be interesting, in the first instance, to give some idea of what may be looked for in comparatively small expanses of water, such, for instance, as the lakes of North America, which, however, exhibit during gales of wind, all the characteristics of an open sea. In the north-eastern corner of Lake Erie, the harbour of Buffalo was constructed at a cost of about L.40,000. It is mentioned in the "Civil-Engineering of North America," that the author "measured (at this harbour) several stones which had been moved; and one of the largest of them, weighing upwards of half-a-ton, had been completely turned over, and lay with its bed or lower side uppermost."

In the Firth of Forth, at the Granton Pier works, on 19th December 1836, after a gale from the north-east, one stone was moved measuring fifteen cubic feet, or about one ton in weight, and thrown on the beach, after having been built into the wall; and a stone containing 18 cubic feet was moved 30 feet from its place; while the *pierres perdues* or mound-stones were washed down to a slope of about 4 to 1.

The following instance, which occurred at the landing slip of the Calf Point, Isle of Man, affords a proof of the great force of the waves even in the Irish Sea. During a gale from the north-west, a block was lifted from its place in the wall and thrown landwards, which measured 123½ cubic feet, equal to about 10 tons weight.

In the German Ocean, we can refer to the Bell Rock Lighthouse,* which, though 112 feet in height, is literally buried in foam and spray to the very top, during ground swells, when there is no wind. It is, therefore, a very important station for making such experiments, because the rise of the spray may be regarded as a scale by which the results of the Marine Dynamometer can be checked or compared.

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* At such a situation as the Bell Rock, a column of water or of air could be conducted into the interior of the house, and might, in the one case, shew the force of each wave as it struck the building by the rise of the water column; or, in the other, by a pressure-gauge, shew the same result in atmospheres by compression.

In the published account of this work there occurs the following statement:— On the 24th October 1819, the spray rose to the height of 105 feet above the rock. "It may, perhaps, therefore," says the author, "be concluded, that the maximum force of the sea at the Bell Rock is to raise the sprays to the height of about 105 feet above the surface of the rock;" and deducting 16 feet, which is the height that the tide rises upon the tower, there is left 89 feet, as the height to which the water is raised. This is equivalent to a hydrostatic pressure of about 2½ tons on the square foot. Since that time, however, there have been still greater proofs of the force of elevation. On the 20th November 1827, the spray rose 117 feet above the foundations or low water mark; and the tide on that day rose 11 feet upon the tower, leaving 106 feet as the height of elevation (exclusive of the trough of the sea), being *equivalent to a pressure of very nearly* 3 *tons per square foot.*

At the island called Barrahead, one of the Hebrides, a remarkable example occurred during a storm in January 1836, in the movement of a block of stone, which, from measurements taken on the spot, is 9 feet × 8 feet × 7 feet = 504 cubic feet, which, allowing 12 feet of this gneiss rock to the ton, will be about 42 tons weight. This great mass was gradually moved 5 feet from the place where it lay, having been rocked to and fro by the waves till a piece broke off, which rolling down, and jamming itself between the moving mass and the shelving rock on which it rested, immediately stopped the oscillatory motion, and thus prevented the farther advance of the stone.

Mr Reid, the principal keeper of Barrahead Lighthouse, the assistant keeper, and all the inhabitants of the little island, were *eye-witnesses* of this curious exhibition of the force of the waves; and Mr Reid also gives the following description of the manner in which they acted upon the stone.

"The sea," he says, "when I saw it striking the stone, would wholly immerse or bury it out of sight, and the run extended up to the grass line above it, making a *perpendicular* rise of from 39 to 40 feet above the high water level. On the incoming waves striking the stone, we could see this monstrous mass of upwards of forty tons weight lean landwards, and the back run would uplift it again with a jerk, leaving it with very little water about it, when the next incoming wave made it recline again. We did not credit the former inhabitants of the island, who remarked that the sea would reach the storehouse which we were building; and when these stones were said to have been moved it was treated with no credit, and was declared by all the workmen at the lighthouse works to be impossible; yet the natives affirmed it to be so, and said if we were long here we might yet see it. They seemed to feel a kind of triumph when they called me to see it on the day of this great storm."

Having now detailed the various observations and facts of which I was possessed in relation to this subject, it may be necessary, in conclusion, to consider the general bearing of such an inquiry.

The advantages which may ultimately arise from a knowledge of the energies of the ocean, can only be guessed at in the present state of our information. It is not to be expected that, in the present train of experiments, much will be found that is directly valuable in practice, as time is required before a true maximum result can be discovered. But a very close and promising connection may easily be traced between the present inquiry and the principles of Hydraulic Architecture, as illustrated in the construction of breakwaters, sea-walls, lighthouses, and piers of timber or of stone, and in the calculations for the strength of the booms which are employed for excluding waves from the interior of harbours; also, in trying the power of waterfalls, and in contrasting the action of waves at the surface with that at the bottom, or at various depths along the sea slopes of breakwaters.

Theoretically, there is much also to invite to a prosecution of such observations. In connection with researches so successfully prosecuted by Mr Scott Russell in the Mechanism of Oceanic Waves, their height, their velocity, and their distance apart, surely observations on the development of the gradually acquired force of such undulations, when they become waves of translation, will form a very important feature in *Marine Mechanics*. In the science of geology, the most direct bearing of the results of the Marine Dynamometer is on the subject of erratic boulders. It is no easy problem to account for the presence of enormous boulders which are foreigners to the formation where they lie, and often, also, far distant from the formation to which they belong. Accordingly we find that glacial action has been suggested as the cause of transportation. Mr Milne has, in the Transactions of this Society, suggested that a continuous rush of waters, due to volcanic emersion, might, at any rate, account for the distribution of the largest erratic boulders which are to be found in Roxburghshire. The results of the Marine Dynamometer, and the facts above recorded of the action of different bodies of water, will certainly be admitted to go far in proof of the competency of aqueous action, to effect the distribution of the erratic blocks referred to by Mr Milne.

Experiments.—With reference to the following experiments I have only to observe, that those which were made at Little Ross, upon the Irish Sea, cannot, from the unusual fineness of the weather at the time, be regarded as affording a true value of the effects of a hard gale in these seas. Of the others it is to be noticed, that where two or three instruments were for some time employed as a check upon each other, and only one or two readings are given, the want has occurred either from the instruments being under repair, or being difficult of access in stormy weather, or during neap tides. It often happened also, in consequence of the springs proving too weak, when new ones had to be made, or the area of the disc reduced. Registers of the state of the weather, apparent height of spray, &c. were generally kept; but it was not considered necessary to complicate the Tables by inserting these, excepting in one or two instances.

Observations at Little Ross.

Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.
1842.		1842.	
April 25	15	June 25	458.0
... 28	51	July 25	380.0
June 1	36	Aug. 2	570.0
... 4	81.5	... 5	665.0
... 20	86.5	... 6	380.0
... 24	840.0	... 9	380.0

The Observations at the Skerryvore Rock, and the neighbouring Island of Tyree, distant 13 miles from the Skerryvore, are as follows:—

Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.
1843.		1843.	
April 24	455	Aug. 9	346
... 25	707	... 22	723
May 7	243	... 30	389
... 11	182	Sept. 5	866
... 12	243	... 21	952
... 16	364	Oct. 5	1535
... 20	495	... 6	1606
	476	Nov. 18	1711
June 3	182	... 19	1497
... 4	519	... 27	1497
... 7	428	... 29	2353
... 8	855	Dec. 5	2674
... 9	173	... 8	3421
July 2	476		At least
... 3	723	... 14	2460
	866	..., 26	1947
... 30	433		

In January, two instruments were placed beside each other, but not set parallel. These instruments had springs of different power, the one being about double that of the other, and one had a disc of 3 inches diameter, the other 6 inches.

Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.
1844.		1844.	
Jan. 6	962	Jan. 9	1925
... ...	928	... ...	1000
... 7	2353	... 10	826
... ...	857	... ...	1000

Both instruments set parallel.

Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.
1844.		1844.	
Jan. 16	428	Jan. 16	427

Another instrument was placed beside them, but the two marked thus * were found to be too weak, as the leathers were found flattened, and one of the instruments was broken, and was not repaired till the 15th February.

Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.
1844.		1844.	
Jan. 28	3422*	Feb. 2	429
... ...	2285*	... ...	457
... ...	3313		

Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.
1844.		1844.	
Feb. 3	429	April 19	800
... ...	457	... ...	535
... 13	214	... ...	481
... ...	228	... 22	913
... 15	321	... ...	428
... ...	280	... ...	962
... ...	321	... 24	1942
... 16	428	... ...	1604
... ...	402	... ...	1370
... ...	343	... 25	1283
... 24	1284	... ...	343
... ...	1364	... ...	321
... ...	685	... 27	457
... 26	2032	... ...	481
... ...	2086	... ...	Night tide 800
... ...	399		
... 27	321	... ...	642
... ...	321	... 30	229
... ...	342	... ...	241
March 4	3316	May 15	343
... ...	3369	... 14	481
... ...	3427	June 6	571
... 7	1069	... 15	1828
... ...	963	July 11	1028
... ...	913	... 13	457
... 10	1925	... 18	914
... ...	1925	... 23	1532
... ...	1713	... 25	571
... 11	535	... 26	971
... ...	481	... 27	457
... ...	456	... 28	1142
... 12	3316	... 29	286
... ...	4011	... 30	914
... ...	2970	... 31	1028
... 13	1142	Aug. 1	571
... ...	1283	... 7	914
... ...	1283	... 13	914
April 10	457	... 14	914
... ...	428	... 21	800
... ...	481	... 30	1713
... 11	800	Sept. 12	1028
... 12	343	... 14	914
... ...	321	... 20	457
... 14	571	... 23	1142
... ...	535	... 25	685
... 16	571	... 30	1599
... ...	642	Oct. 2	2399
... ...	481	... 3	1485
... 17	800	... 4	1828
... ...	856	... 11	3427[1]
... ...	962	... 14	1599
... 18	571	... 19	1599
... ...	481	... 20	2513

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[1] On this occasion, 14 stones were slightly moved, and 14 scattered, all of which had been built into the round-head or end of Hynish Pier, which was still in an unfinished state, and a Dynamometer which was attached to the Pier, registered on this occasion 2557 lb. These stones weighed from 1 to 1½ tons, and exposed, when built into the wall, about 2 square feet of surface. The stone to which the instrument was fixed was turned upside down, although it weighed about 1¼ ton = 2800 lb.

Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.
1844.		1844.		1844.		1844.	
Oct. 22	800	Nov. 10	1028	Nov. 23	3427	Dec. 10	1825
... 24	1827	... 14	1257	... 27	3199	... 13	1925
... 28	1485	... 15	2056	... 28	4112	... 14	1027
... 29	457	... 16	2056	Dec. 7	1369	... 15	1764
Nov. 2	1942	.. 22	2627	... 9	2738		

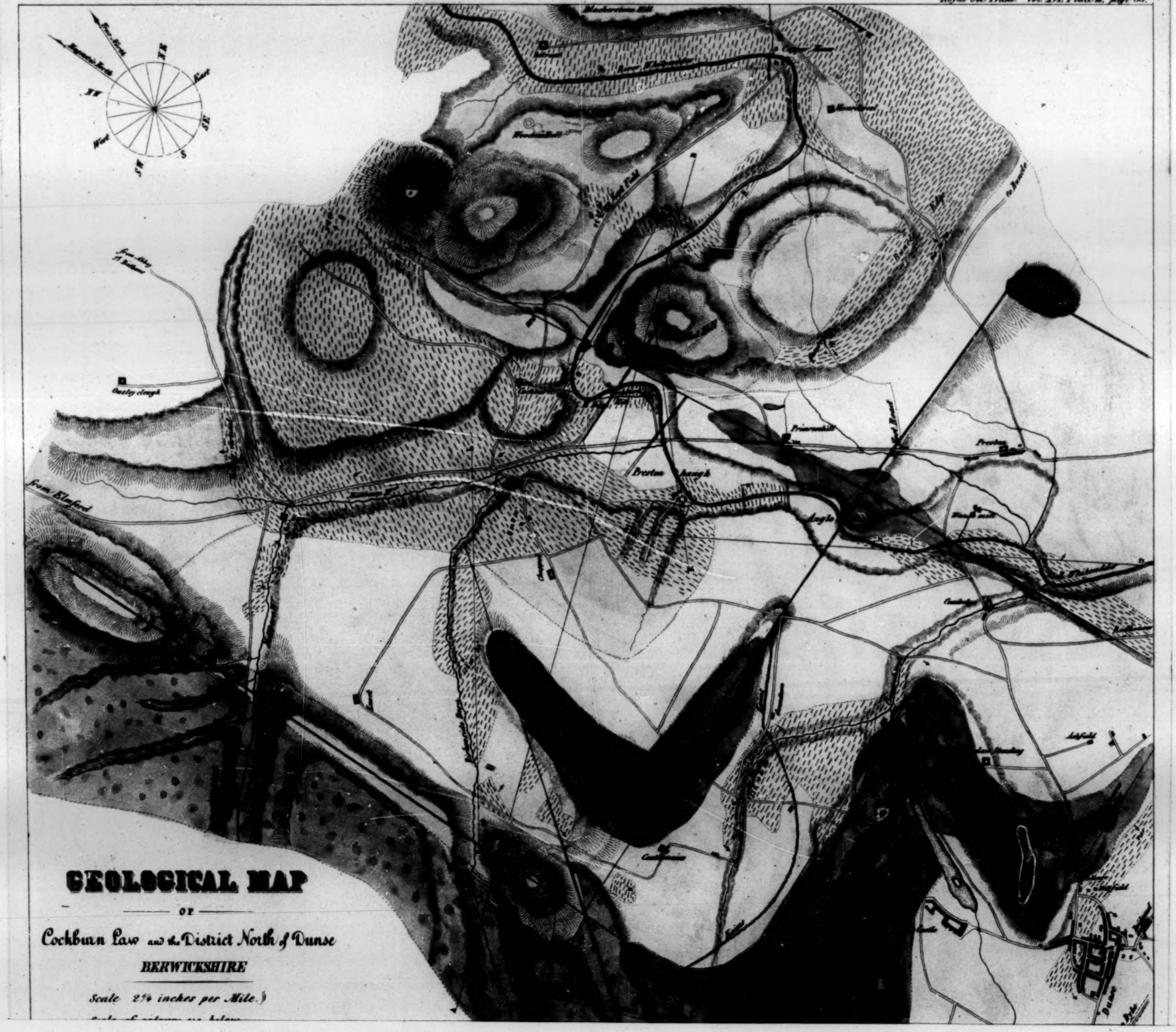
A more exposed point of the Skerryvore Rock was at this time chosen for experiment; and with the view of ascertaining the effect of the waves at different heights upon the rock, two instruments were fixed, the one (No I.) several feet lower, and about 40 feet seaward of the other (No. II.) It was observed, that about half-flood the force of the waves was a good deal expended before they reached the place where No. I. was placed, from there being so little water on the rocks outside. Whereas when the tide was higher the waves were, from the greater depth of water, not so much broken when they reached No. II. The results of the Marine Dynamometer shew generally about twice the force at No. II. as at No. I.; a result which shews how important it would be to ascertain the relative forces of the waves at different levels upon our breakwaters and other seaworks.

Date.	Remarks.	No. of Instrument.	Pressure in lb. per Square Foot.
1845.			
Jan. 7	Heavy sea.	I.	1714
... ...	... ...	II.	4182
... 12	Very heavy swell.	I.	2856
... ...	... ...	II.	5032
... 16	Heavy ground swell.	I.	2856
... ...	... ...	II.	4752
... 22	A good deal of sea.	I.	2856
... ...	... ...	II.	5323
... 28	Heavy ground swell.	I.	2627
... ...	... ...	II.	4562
Feb. 5	Fresh gales.	I.	856
... ...	... ...	II.	3042
... 21	... ...	I.	1827
... ...	... ...	II.	3422
... 24	Fresh breezes.	I.	1256
... ...	... ...	II.	3802
March 9	Ground swell.	I.	1256
... ...	Waves supposed about 10 feet high.	II.	3041
... 11	Short sea.	I.	1028
... 24	Heavy sea.	I.	2281
... ...	Waves supposed about 20 feet high.	II.	4562
... 26	Swell.	I.	1256
... ...	Waves about 6 feet high.	II.	3041
... 29	Strong gale, with heavy sea, the highest waves supposed 20 feet high, and the spray rose about 70 feet.	I.	2856
		II.	6083

Register of Observations on the force of the Sea, made at the Bell Rock, German Ocean.

Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.
1844.		1845.	
Sept. 15	853	Jan. 27	1199
... 20	2260	... 30	2879
Oct. 9	3013	... 31	1559
... ...	2562	Feb. 6	2999
... 26	1142	.. 24	1199
... 27	958	... 25	959
Nov. 12	1680	... 27	839
... 13	1920	... 28	1319
Dec. 13	1560	March 4	959
... 26	1439	... 7	1079
		... 11	1919
1845.		... 20	2519
Jan. 7	1559	... 21	2759
... 10	1439	... 24	1319
... 11	1439	... 25	959
... 15	1559	... 28	599
... 25	959	... 30	1079
... 26	719		

Royal Soc. Trans. Vol. XVI. Plate II, page 33.



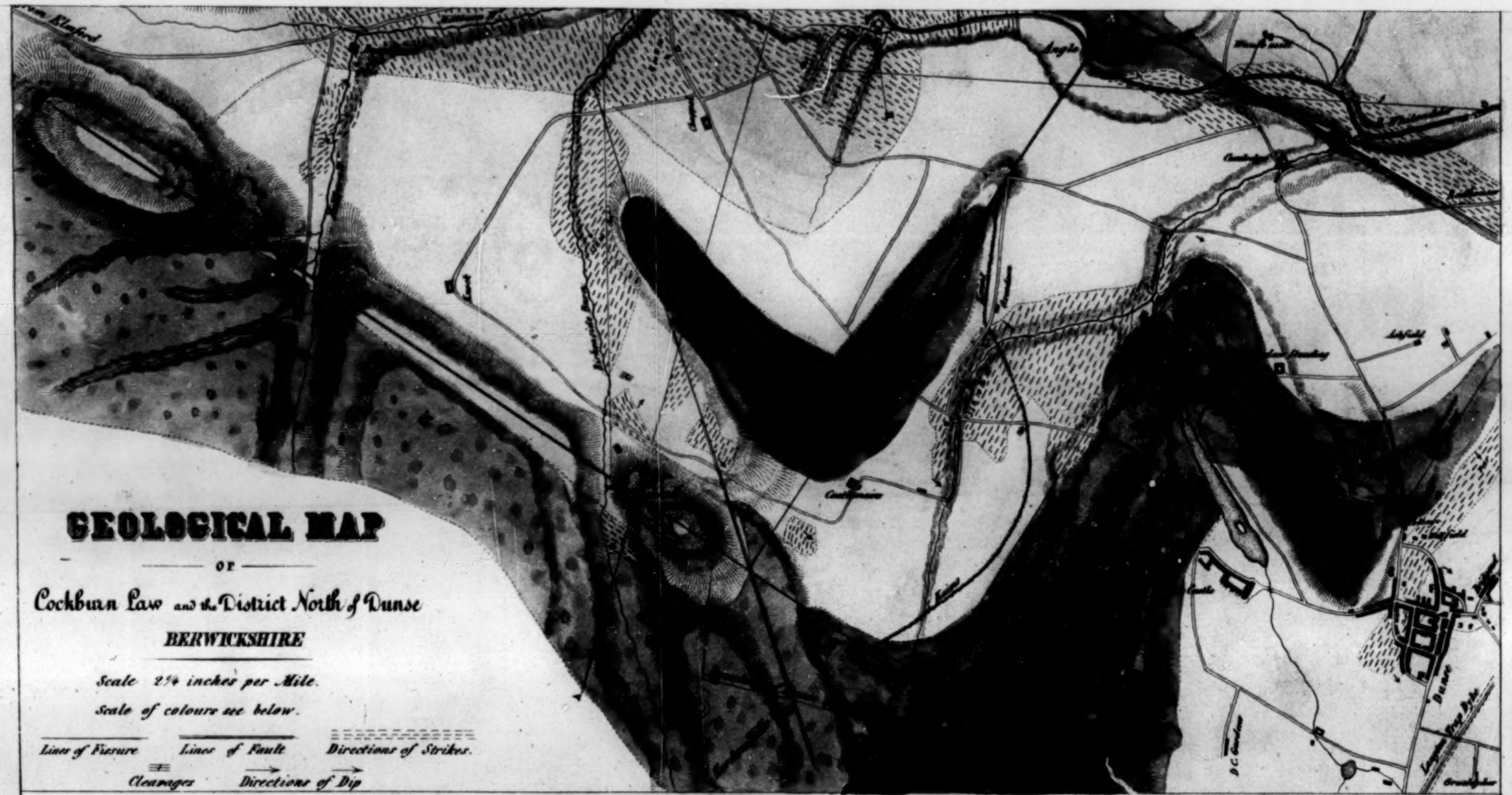
GEOLOGICAL MAP

OF

Cockburn Law and the District North of Dunse

BERWICKSHIRE

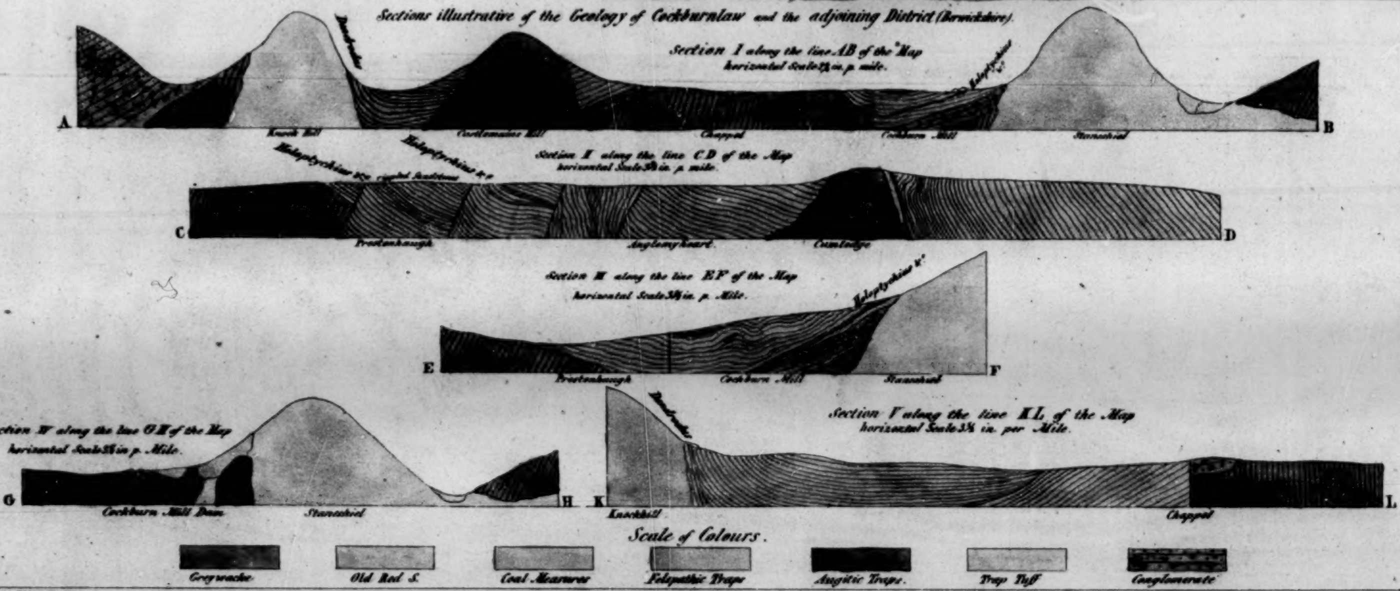
Scale 2¾ inches per Mile.



GEOLOGICAL MAP
OF
Cockburn Law and the District North of Dunse
BERWICKSHIRE

Scale 2¼ inches per Mile.
Scale of colours see below.

Sections illustrative of the Geology of Cockburnlaw and the adjoining District (Berwickshire).



Section I along the line AB of the Map
horizontal Scale 3⅞ in. p. mile.
A
B
Knock Hill
Castlemains Hill
Chappel
Cockburn Mill
Stanechiel
Section II along the line CD of the Map
horizontal Scale 3⅞ in. p. mile.
Holoptychius &c.
Holoptychius &c. in red Sandstone
C
D
Prestonhaugh
Anglemyheart
Cumledge
Section III along the line EF of the Map
horizontal Scale 3⅞ in. p. Mile.
Holoptychius &c.
E
F
Prestonhaugh
Cockburn Mill
Stanechiel
Section IV along the line GH of the Map
horizontal Scale 3⅞ in p. Mile.
G
H
Cockburn Mill Dam
Stanechiel
Section V along the line KL of the Map
horizontal Scale 3⅞ in. per Mile.
K
L
Knockhill
Chappel
Scale of Colours.
Greywacke
Old Red S.
Coal Measures
Felspathic Traps
Augitic Traps.
Trap Tuff
Conglomerate

Fr. Schenck, Lithr. Edinr.

17 MR 58

V.—*On the Geology of Cockburnlaw, and the adjoining District, in Berwickshire; with a Map and Sections.* By William Stevenson, Dunse.

The portion of Berwickshire, of which the object of this paper is to describe the more remarkable geological features, comprises, within a very limited space, an assemblage of phenomena of the highest interest, in relation to the wonderful changes which this part of our island has undergone in the earlier periods of the earth's history. Its situation is immediately to the north of Dunse, and its area is about 16 square miles, within which are comprehended the junctions of the greywacke rocks of the Lammermuirs, with the strata of the old red sandstone formation, and of the latter with the lower members of the coal measures. The chief eminences within the district are Cockburnlaw, which attains a height of 912 feet above the level of the sea; Dunselaw, 630 feet; the Knock, Borthwick, and Castlemains Hills; and the Staneshiel, a hill of the same formation as Cockburnlaw, from which it is separated by a ravine of 300 or 400 feet deep, in which the Whiteadder flows. The general topographical features of the district will be best understood by reference to the accompanying map.

In treating of our subject, we propose to describe, 1*st*, The *stratified* rocks of the district; and, 2*dly*, The *igneous* rocks, and the changes of structure and position which have been effected by their agency upon those of aqueous origin.

I. Stratified Rocks.

1. *The Greywacke of the Lammermuirs.*

The strata of this important series of deposits (the most ancient of the district under consideration) are finely exposed in many localities, consisting of arenaceous and argillaceous beds, of great, but as yet, unascertained thickness, the former constituting the *greywacke* proper, and the latter the *greywacke slate.* The oxides of iron and manganese are widely diffused among these strata. Magnesia enters pretty largely into their composition; but there seems to be an almost entire absence of calcareous matter, there being no beds of even impure limestone interstratified with the greywacke, and but a small quantity diffused through its masses. Sulphate of baryta is also very generally met with, in the form of veins of various sizes up to a foot or more in width, and occupying fissures in the greywacke. Near Elmford, the cleavage fissures of the greywacke strata are filled with this mineral.

No decided traces of *organic remains* have hitherto been discovered in the greywacke of this part of the Lammermuirs. On some slabs from a quarry near Hoardweel, some curious relieved markings appear, which may prove to be of organic origin; but as no trace of organic matter is observable in these specimens, it is uncertain whether they are not merely the effects of a chemical aggregation.

2. *The Old Red Sandstone.*

The formation which in this district immediately succeeds the greywacke, is the *upper division* of the old red sandstone, there being no strata on the flanks of the Lammermuirs, referable to any of the intermediate formations, comprisingthe Silurian system, and the lower and middle divisions of the old red sandstone. The position of the red sandstones and conglomerates, with reference to the greywacke, is unconformable. This is very distinctly seen at Cockburn Mill, where the conglomerate, which constitutes the lowest member of the formation, is well exposed, overlying the fractured ends of the greywacke strata in such a manner that the planes of stratification of the two sets of rocks are nearly at right angles to each other. The greywacke strata here are of the red variety, and dip at high angles to NW., the vacancies between their uneven and broken ends being filled up by the conglomerate. The latter is chiefly composed of fragments of the subjacent red greywacke, together with pieces of the felspathic rocks of the Staneshiel; the whole being firmly cemented by the finer arenaceous particles derived from the adjoining rocks.

The variations in the thickness of the conglomerate at different localities of this limited district are very great, and would appear to indicate the proximity of the shore, at the period of its deposition; a view which other circumstances, to be hereafter detailed, tend strongly to corroborate. At Cockburn Mill, its thickness may average about 20 feet; whereas to westward of the Knock hill, it is at least 300 or 400 feet. At the latter place, it contains (besides fragments of greywacke, which are its chief ingredients) several varieties of felspar porphyry, together with rolled masses of quartz, hæmatite, and other minerals. In Kidshielhaugh, and near the Knock hill, it consists almost entirely of fragments of the felspathic rock, which occurs *in situ* at these places. These are cemented by calcareous spar, which has probably been derived from the igneous rock, as the latter contains a considerable quantity of carbonate of lime in its composition. Similar instances of calcareous matter acting as a cement to the conglomerate, are not unfrequent on the borders of the Lammermuirs.

At Cockburn Mill, the conglomerate passes, by a series of alternating conglomerates and sandstones, into the characteristic strata of the formation. These consist of red and greenish-white sandstones, which alternate with red clays, the thickness of the whole of which, as exposed in Prestonhaugh, is probably at least 300 feet; but, on account of the shattered nature of the strata, this cannot be exactly ascertained. Much interest attaches to these strata on account of the organic remains, and the curious markings which appear to be of organic origin, in which they abound. These we shall attempt to describe as concisely as possible.

Organic remains were first discovered among these sandstones, in the summer of 1840, in a fine section exposed by the Whiteadder about half a mile below Cockburn Mill. These chiefly consist of scales, occipital plates, ichthyodorulites, and other bony parts of the *Holoptychius Nobilissimus*. Similar remains also occur in the sandstones directly opposite to Cockburn Mill, and in those to the east of the Knock hill. They are not distributed uniformly through the strata in which they occur, but are found only in particular beds, in which they abound; while very few or none are to be seen in the adjoining strata. At Cockburn Mill, the ichthyolitic beds are situated within a hundred feet of the conglomerate, from which they are separated by a series of intervening beds of unfossiliferous red sandstones, and thin strata of conglomerate. One bed which is exposed here contains the remains of the Holoptychius in such abundance, that a chip cannot be struck off without disclosing a portion of a scale or plate. It is of a coarse, sandy, gritty texture, and is generally so brittle from being highly impregnated with animal matter, as to be easily broken between the fingers. It is only a few inches thick. Some of the thicker beds with which it is associated also contain these fossils in considerable abundance, but they appear to be in greater quantity *near the surfaces* of each stratum, few being found in the interior of a thick bed. These strata present beautifully rippled surfaces, and (especially the gritty bed above mentioned) shew other unequivocal marks of littoral deposition. A circumstance which tends strongly to favour the view of their having been formed near the shore, is the fact of their included remains being of such a fragmentary character, that although pieces of scales and plates may be picked up in hundreds, it is very rare to find one of either that is not more or less mutilated. Indeed, it would appear that after those large fishes died, their osseous parts, being separated by decomposition and the action of the waves, were tossed about on the sandy beach, and exposed to attrition among the coarse sand and pebbles, until they were reduced to the fragmentary state in which we now find them. The fact of the remains of the Holoptychius being chiefly found associated with appearances indicative of the proximity of the shore, would almost lead one to suppose that these fishes were in the habit of frequenting shallow water, perhaps because of their food being there more abundant. At all events, their remains appear to be confined to these rippled strata; for in the corresponding beds of the old red sandstone above Greenlaw, which are destitute of ripple marks, and bear evidence of having been deposited in deep water, there are no traces of them to be found.

On the east side of the Knock hill, the ichthyolitic beds of this formation are tilted up at high angles by the mass of grey porphyry of which that hill consists. The remains found here are of the same fragmentary character as those of Prestonhaugh, and abound most in a few thin sandy beds within a few feet of the porphyry. They consist chiefly of the scales, plates, spines, and teeth of a fish allied to the Holoptychius, but differing from it in several particulars, the scales being more plainly marked, and the plates of a different shape and style of sculpture, being *reticulated* instead of *tuberculated.* The teeth are beautifully fluted, and appear to belong to the *Dendrodus striatus* of Professor OWEN, to which fish the associated scales, plates, and spines probably also pertained.

In Prestonhaugh there are found, besides the remains of the Holoptychius, other relics of a more obscure and puzzling, but highly interesting character, and seem to be of organic origin, though all trace of their original organic matter has which disappeared. These present many varieties of size and form, the most common being spindle-shaped bodies, which project in relief from the surfaces of the strata in which they occur. These are generally about half an inch long, detached, or connected by threadlike ridges. Another sort resembles in form and size the crysalis of a butterfly, while others present a vermiform appearance. It seems probable that the majority of these curious markings are nothing else than the petrified forms of soft-bodied animals that crawled in the mud of those ancient shores, and which, being covered by a deposit of sand, left the outline of their forms impressed thereon, the gases evolved in the process of decomposition escaping through the sand, and being replaced by the infiltration of the finer particles of mud. Others are perhaps coprolitic, and some, probably, mere concretions. The appearances presented by the bottom of a shallow pool which has been recently dried up, afford an apt illustration of some of these markings, the smooth glazed surface of the mud being marked here and there with worm-pits and castings, and furrowed by the traces of worms and insects, while it is divided into irregular portions by fissures of desiccation. In fact, some of these recent markings are almost perfect fac-similes of those which occur among the rippled strata of Prestonhaugh. Nor is evidence awanting among these strata of fissures having been produced by desiccation. There is one bed, in particular, of whitish sandstone, overlying strata of a softer and more clayey character, the under surface of which, wherever exposed, is seen to be entirely covered with relieved mouldings, which ramify in all directions, forming a sort of irregular net-work, and in short, exactly resembling the appearance which would be presented by the under surface of any plastic or molten substance poured into the cracks produced in mud by the heat of the sun. The depressions between these reticulated mouldings are generally smooth and shining, being coated with a fine red clay. Scales of the Holoptychius also occur pretty frequently on the same surface.

These interesting strata are succeeded by others in which the clays rather predominate, and which seem to be quite destitute of organic remains. They are, however, profusely marked with greenish-white spherical spots, of various sizes, from that of a pea, up to several inches in diameter. The colouring matter of the sandstone (the peroxide of iron) appears to have been discharged by some powerful deoxidising process, probably the putrefaction of animal or vegetable matter. Scales of the Holoptychius are sometimes met with, surrounded by a blanched space, the breadth of which seems to be *less* in proportion to the *better* preservation of the scale, and *vice versa*. Where organic remains are most abundant, the strata are blanched in large irregular patches. In the centre of the spots a small speck of protoxide of iron may be found, but this is generally so minute, as not to be visible, unless the plane in which the spot is split passes through the centre.

The only decided *vegetable* remains found in Prestonhaugh, occur in some beds of soft red sandstone, overlying the spotted strata above described. The matrix in which they are embedded has been unfavourable to their preservation, so that they are obscurely marked, and when exposed to the weather for a short time, become almost indistinguishable. They appear to be a sort of *Algæ*. At Cockburn mill, some slabs of rippled sandstone present markings in relief much resembling *fucoids*, but shew no trace of carbonaceous matter, so that the vegetable origin of these, though not unlikely, is at least doubtful. Great numbers of the vermiform bodies before described are associated with these markings.

The strata in Prestonhaugh containing the vegetable remains, are succeeded by other beds of red and variegated sandstones and clays, marked with blanched spots and ripples, and containing, at least, one stratum in which the remains of the holoptychius are very abundant. Among these also occur thin beds of coarse sand, mixed with small rounded pebbles of white quartz, and presenting decided indications of littoral deposition. A succession of red sandstones and clays, which appear to have been deposited in deep water, follow, and may be traced along the south bank of the Whiteadder to Preston bridge, where they are interrupted by a large trap-dyke, accompanied by a very extensive fault or dislocation. On the opposite side of this dyke the coal measures appear tilted up at high angles, there being only the breadth of the dyke, which at this place does not exceed 100 yards, between the two formations. In consequence of this dislocation, and subsequent denudation, the upper beds of the old red sandstone, or those which graduate into the coal measures, are awanting at this place, having been entirely swept away on the west side of the dyke; while, on the east, they are buried beneath a great thickness of the coal measures.

The transition rocks thus removed or thrown down at this place, are pretty well developed in other parts of the county; as in the course of Langton burn, about two miles SW. from Dunse, and in that of the Blackadder above Fogo. They consist of thick beds, of a coarse, arenaceous, concretionary limestone, or cornstone, with which are associated beds of shale and clay, also impregnated with a considerable quantity of calcareous matter; and as regards colour and general aspect, blending the characters of the argillaceous strata of the old red, and the shales of the coal measures. They appear to be destitute of organic remains, either animal or vegetable, and exhibit no appearances of ripple marks or blanched spots.

3. *The Coal Measures.*

This important formation is represented in this district, by a small patch east of the Cumledge trap-dyke. Its strata are well exposed in the bed of the Whiteadder, near Preston bridge, about a quarter of a mile below which, are two fine sections. There the sandstones, shales and clays, which characterize the formation, are seen dipping away from the dyke at high angles, and presenting the usual appearances by which this series of strata is elsewhere distinguished. The shales and clays contain a large proportion of carbonate of lime in their composition; so much, indeed, that they might almost be termed *limestones*. The sandstones are of the usual white or yellowish colour, in many places highly micaceous, and abounding in impressions of Stigmariæ, Sigillariæ, Lepidodendra, and other plants of the carboniferous system. There is no appearance of *animal* remains.* No *coal* seams appear, but there is a thin stratum of ironstone in nodules, which abounds in remains of plants. The geological position of these strata, and the other rocks of the same formation, which prevail in the Merse of Berwickshire is considerably below the *Encrinal limestone*, which crops out near Berwick, and on the sea-shore at Lamberton. Indeed, they properly belong to the mountain limestone series, being situated far below the true coal measures. From their very low position in the series, there is no reason to suspect the existence among the Berwickshire strata of any coal-seams sufficiently thick to be worth working.

II. IGNEOUS ROCKS OF THE DISTRICT, AND THEIR EFFECTS UPON THE SEDIMENTARY ROCKS.

Throughout the district under consideration, trap-rocks are very abundantly distributed, and present a field of speculation, no less attractive than those of aqueous origin. The traps of the Lammermuirs and adjoining districts belong to two great classes, differing from each other, as well in mineral character and general aspect, as in regard to the epochs of their eruption. These are the *Porphyries* and *Greenstones*, or *Felspathic* and *Augitic* traps, both of which classes are very abundant, but (with one or two exceptions, to which we shall afterwards have occasion more particularly to refer) do not occur associated with each other; for it is a remarkable fact, that while the traps which occur among the greywacke of the hills *are uniformly of the Felspathic class*, those which appear in connection with the secondary strata along the flanks of the Lammermuirs, *belong as exclusively to the Augitic family*. In considering these, we shall follow the natural order, by describing, 1st, the Felspathic traps, and their effects upon the greywacke strata; and, 2dly, the *Augitic*, or more modern traps, and the effects which they have produced upon the more ancient rocks, both stratified and unstratified.

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* Within the last few weeks the remains of fossil fishes have been discovered in the course of Langton Burn, about a mile SW. from Dunse, in strata belonging to the lower part of the coal measures. These remains consist of scales, spines, teeth, and other bones, similar to those found at Burdiehouse. They occur in a soft friable sandstone, which abounds also in *Lepidodendra*, and other plants of the coal formation. Some remains of the *Holoptychius* have likewise been recently found in the old red sandstone strata on the estate of Billie, about four miles NE. from Dunse.—21*st April* 1845.

1. *Felspathic Traps.*

Rocks of this class are very abundant among the Lammermuirs, generally occurring in the form of dykes or veins of various sizes, intersecting the greywacke; or in large masses constituting entire hills. They consist of two or three varieties of granite and syenite, with an almost endless variety of felspars, claystones, and felspar porphyries. The following are some of the principal rocks of this kind which occur in the district.

Granite of Cockburnlaw and Staneshiel.—This rock shews a great variety of aspects at different parts of these hills. Near the *outskirts of the mass*, where it is in contact with the greywacke, its constituent crystals are very small, and its cleavage structures rectangular, and according with the strikes and cleavages of the adjoining strata. On proceeding into the interior of the mass, however, the rectangular cleavages due to its refrigeration, in accordance with certain lines, are superseded by the structures resulting from the crystalline tendencies of the granite, which have of necessity had more time for their proper development, in proportion as the distances from the cooling surfaces increased. The rock, in consequence, becomes harder, and its crystals larger and better defined, while it is divided by its structural planes into large, irregular, pyramidal blocks. Its most common character is that of a regular granite, composed of distinct crystals of white quartz, red felspar, and black mica; being identical, both in regard to geological age and lithological aspect, with a granite which is associated with greywacke near Fassney Bridge; and which, from being intruded among the strata of the latter, in conformable beds, caused much discussion between the Huttonians and Wernerians. In some places, as, for example, on the south side of the Staneshiel, about half way up the hill, sulphate of baryta is added to the usual ingredients of the granite; and near the top of that hill small quartz veins occur, containing *galena* and *copper pyrites*, though in very minute quantity On the left bank of the Whiteadder, about 60 yards below Cockburn mill-dam, a mass of syenitic rocks of extreme hardness appears in contact with the greywacke, and projecting into the bed of the river. This is evidently a process from the Staneshiel hill, the granite of which, as well as that of Cockburnlaw (which is indeed a part of the same mass), in many places passes into syenite. In fact, after an attentive consideration of the phenomena presented by the transition of one rock into another, and especially the changes effected upon both aqueous and igneous rocks, at and near their junctions, of which many highly instructive examples are seen in this district, it seems not improbable that the *syenite* of Cockburnlaw and the Staneshiel is nothing more than *greywacke*, *fused* by the agency of the molten granite, and the mineral characters of the two rocks thereby blended together. The granite invariably assumes the aspect of syenite, as it approaches the greywacke. At Cockburn Mill dam, the greywacke is considerably hardened, and dips to NNW. at high, but varying angles. Below the dam the hardness increases, the planes of stratification become less distinct, while those of cleavage grow more decided at every step. All these symptoms of metamorphism increase as we approach the igneous rock, the texture of the greywacke being changed to crystalline, and the size of the crystals increasing with the hardness, until we arrive at a point, where it is impossible to decide from the appearances presented, whether the rock should be considered *greywacke* or *syenite*. Beyond this, it graduates into true syenite, which is divided by cleavage planes into large rectangular blocks, arranged in the form of thick beds, having he same dip and strike as the adjoining greywacke. The cleavage planes which run *parallel* to those of the stratification of the greywacke (if, indeed, these are not merely the original planes of stratification of the rock before it was converted into syenite), are distinguished from those *running at right angles* thereto, by being occupied by veins of *heavy-spar*, associated with crystals of quartz. These veins run continuously in a WSW. to ENE. direction. As we proceed further into the mass, the syenite becomes more crystalline, and passes by a regular and gradual transition, into the well characterized granite of the Staneshiel. Two varieties of granite are here seen intruding in the form of veins, which have evidently been poured, in a molten state, into fissures opened in the syenite, during the process of crystallization. One of these is a beautiful and regularly crystallized granite, larger in the grain than is commonly met with in the adjoining hills. The other is small grained, and rather soft, and seems to be connected with a dyke which occurs in the bed of the river a little above the dam, being identical in mineral character. This dyke can be traced, when the water is low, for about 200 yards. It is about five feet thick, and runs nearly NNW., but is frequently interrupted in its course by the greywacke which it crosses. It contains, in some places, fragments of the latter rock, which it has detached and brought up with it, in its passage through the strata. The adjoining strata have been partially fused, and present in some places a syenitic appearance. The alteration extends to a considerable distance from the dyke, the strata being extremely hard, and frequently exhibiting contorted laminæ. In the immediate vicinity of the dyke the planes of stratification are very obscure, being, in many places, merely marked by veins of spar. Both the dyke and the adjoining metamorphic rocks are traversed by numerous veins of *heavy-spar*, together with a few of quartz.

The summit of Cockburnlaw consists of beds of metamorphic greywacke, which dip to NW. at an angle of about 65°. The metamorphism of the strata increases as we approach the great body of granite, which lies immediately below the summit to SE. They here present a highly crystalline and *syenitic* appearance; and, in hardness, exceed even the granite itself, on the shoulder of which they have been elevated to their present position.

Associated with this granite are several varieties of porphyry, belonging to the same geological epoch. These appear at short intervals in the bed of the river, from the eastern boundary of the granite, as far as Abbey St Bathans, a distance of about four miles. The granite of which the steep hill opposite Cockburn-eastfield consists, is seen at the brink of the river to pass into a kind of porphyry, consisting chiefly of whitish felspar, with crystals of dark coloured mica. Another variety of porphyry, which also occurs at a short distance from the eastern margin of the granite, is of a deep red colour, derived probably from the peroxide of iron. But the most common variety is a porphyry, having a basis of cream-coloured felspar, with disseminated small crystals. Of this Blackerstone hill is composed; and it may be seen at intervals between the east side of the granite and Hoardweel, underlying the greywacke. At some places, where the latter rests immediately upon the porphyry, it appears to have been actually fused, having lost every semblance of its original stratified structure, and being divided, like the subjacent porphyry, into extremely sharp pyramids and wedges. In some places, especially where it comes in contact with the whitish porphyry before mentioned, the greywacke has, in the process of fusion, become blended with the igneous rock, forming a curious mongrel sort of compound.

At the bend of the river, below Hoardweel, the porphyry is seen penetrating the greywacke in the form of conformable dykes. From this place to the copper mines, the channel of the river is narrowed by vertical rocks of metamorphic greywacke. At the "Strait Loup" it rushes through a gorge so narrow that it may in general be easily stepped over. The geological phenomena displayed here are very interesting. Within a space of about fifty yards by thirty, the porphyry has forced its way through the strata in eleven or twelve different places. The greywacke is much hardened and contorted; and, near the contact with the igneous rock, becomes cupriferous, and abounds in quartz veins. The copper ore, which is of the green and grey varieties, occurs in the schists which alternate with the greywacke. The porphyry is generally of the same kind as that further down the river; but in some places passes into a greyish-white compact felspar; and, in others, becomes a kind of granite (the felspar, however, predominating), which contains disseminated specks of iron pyrites. In some instances it forms dykes, which are, to a certain extent, conformable with the greywacke; but it commonly occurs in irregular masses of small extent, lying among the disturbed strata, and connected with each other by veins or dykes.

The metamorphism of the greywacke is observed invariably to take place in the vicinity of the granite and associated porphyries; and the process can be traced in a most satisfactory manner, through all its stages, in many places among these hills. At several localities, series of specimens may be obtained, shewing the gradual transition from the unaltered greywacke, to where it becomes converted into syenite, which again passes into granite; and the latter, in its turn, graduates into porphyry,—the progress of transformation being so gradual, as to render it very difficult to decide where the characters of the *aqueous* rock become merged in those of the *igneous*. In many cases, the only way of distinguishing the original planes of stratification from those of cleavage, which have been superadded in the metamorphic process (unless, indeed, we can trace the strata uninterruptedly from where they are unaffected), is by the schists which alternate with the greywacke. These are sometimes much contorted, and present the appearance of hornblende slates.

While the greywacke is thus altered, wherever it approaches the granite or porphyries, the old red sandstone strata, being of a date subsequent to that of the eruption of these older traps, are unchanged even at the place of contact. This is seen on the SW. side of the Staneshiel, where the red sandstones are *tilted up against the granite at high angles, without any appearance of alteration*,—the granite and overlying sandstones having, in this instance, been elevated (as we shall hereafter shew) by the agency of the *augitic* traps.

Porphyry of the Knock Hill.—This rock is of a grey colour, having a felspathic basis, and containing a considerable quantity of carbonate of lime. The hill to westward of Burnhouses consists of the same rock, and is connected with the Knock hill by a ridge running in a SSE. direction. To westward of the felspathic mass of the Knock hill, the greywacke appears in nearly vertical strata, with a NNW. strike, and is considerably altered by the proximity of the igneous rock, being hard and full of quartz veins. On the east side of the hill, and within a few feet of the porphyry, the red sandstones are seen tilted up at high angles, and even partially retroflexed; but being of more modern date (as in the case of the junction of the granite and sandstones on the west side of the Staneshiel), they shew no traces of metamorphism, but remain quite fresh and soft; and the remains of scales, &c., of the Dendrodus which they contain still preserve their original colour. Similar appearances are also observed in the vicinity of the porphyry in Kidshielhaugh.

2. *Augitic Traps.*

Rocks of this class also abound in the form of dykes, beds, and irregular masses, and consist of several varieties of greenstone, basalt, amygdaloid, and trap tuff. These have all been erupted subsequent to the deposition of the old red sandstone and lower coal measures, as is evident from the disturbance and metamorphism apparent in these strata, whenever they approach to traps of this class. With one or two exceptions (to be hereafter noticed) they do not appear in contact with the older rocks, being confined to the secondary strata, which are much disturbed by their agency. Besides the veins and masses which are exposed, there are, undoubtedly, many veins and extravasated portions concealed among the strata. The principal masses of these traps which occur in the district are the following:—

The *Cumledge trap-dyke* is a large body of trap of irregular thickness, which is seen in the bed of Oxendean Burn, near Cumledge House, and about 200 yards from the Whiteadder. It is here a sort of amygdaloidal greenstone, abounding in veins of zeolite, steatite, and other minerals, and is probably not more than ten yards in thickness. On the west side it appears in contact with beds of a sort of cornstone, which are excessively hard and crystalline at the junction, while the trap becomes soft and earthy. On the east side a similar description of rock occurs. From this spot to the Whiteadder the strata are hid by debris, but appear to belong to the coal measures, of which a fine section is presented a short way below the place where the burn joins the Whiteadder, in a cliff of more than eighty feet high. The shales, sandstones, and clays, are here seen dipping away from the dyke at angles, which increase in proportion to the proximity of the line of disturbance, until they become vertical, and even partially reversed. From this place to Preston Bridge, the strike of the strata is very regular, and parallel to the course of the dyke, which runs in a SSE. direction. At Preston Bridge its thickness is upwards of 100 yards. The strata of the coal-formation come close up to it on the east side; while on the west it cuts off the old red standstone. On the north bank of the river, about 300 yards above the bridge, it is seen in contact with strata of whitish sandstone and grey calcareous shales, which seem to be equivalents of those strata which elsewhere constitute the transition beds between the old red sandstone and coal measures. From this place it may be traced in the bed of the river, presenting, in general, the aspect of an amygdaloid, until we reach a place called "Anglemyheart," where it passes into a beautiful columnar basalt. The columns are irregular hexaedral prisms, not arranged vertically, but dipping at high angles to west. The basalt is more crystalline, and the columns more regular towards the interior of the mass. Towards the outskirts it passes into an olive-coloured greenstone; and the latter graduates into a trap-tuff, composed of fragments of greenstone and metamorphic greywacke, agglomerated into a mass.

This trap-dyke appears to run under the granite of the Staneshiel and Cockburnlaw,—not, however, *directly under the centre* of the granitic mass of these hills, but *more to the west side.* In the glen or ravine between these two hills, and exactly in the line of continuation of the Cumledge dyke, a mass of basalt occurs, which has burst through the older rocks. There is every reason to believe that an eruption has taken place from a crater at this locality, and that the basalt now occupies what was formerly a volcanic vent. A considerable quantity of trap-tuff is seen in the bed of the river, in the vicinity of the basalt, and at intervals for about 300 yards farther down, wherever it has been protected from denudation. It consists of a coarse sand (apparently triturated granite), containing rounded nodules of various sizes, from that of a pigeon's egg upwards, formed of concentric coatings of the granitic sand round a nucleus, which appears to consist of an imperfect sort of basalt.

The basalt is of a dark colour, approaching to black, and is very hard. It is occasionally amygdaloidal, and has a tendency to assume the form of concretionary masses of a spheroidal figure, consisting of concentric coatings. The granite is seen in contact with it on the north side,—the line of junction running in an ENE. direction. On the south side the two rocks are separated by a deep pool in the river, which has probably been formed by the washing out of the incoherent tuff which here enwraps the basalt. On the west side of the basalt is a very hard rock resembling syenite, and which is probably greywacke, fused by the igneous rock. Its cleavages, as well as those of the granite and basalt, are ENE. by NNW.

In the interesting section exposed by the Whiteadder at Cockburn Mill, a bed of trap about 4 feet thick is seen overlying the old red sandstone strata for a considerable extent. It is of a brown colour, and abounds in vesicles generally about the size of small peas, some of which are empty, but the majority are filled up with various minerals. Its texture is earthy, and it is much debased by being mixed with the debris of the adjacent sandstones and clays, portions of which it had taken up in its course when in a molten state. The embedded fragments are hard, crystalline, and cherty, and the adjoining strata are discoloured, and their laminæ contorted, shewing very clearly the effects of igneous action. As this bed of trap appears to be destitute of augite, it should, mineralogically, be classed with the porphyries and other felspathic traps of the Lammermuirs. It is, however, more recent than these, having been erupted subsequent to the deposition of at least the greater part of the old red sandstones. At the same time, it seems to be more ancient than the augitic traps by which the second upheaval of the Lammermuirs was effected, as it shews some appearances of having participated in that movement along with the adjoining sandstones.

The trap of Castlemains hill is a mass of greenstone, which has forced its way through the old red sandstone strata. The sandstones are much hardened at and near their junction with the trap, while the same beds are seen within 300 yards to NW., *in contact with, and tilted up by,* the grey felspathic rock of the Knock hill, without the least appearance of alteration. The latter circumstance which seems rather anomalous, is accounted for by a dyke of augitic trap, which runs from under the Knock hill in a SSE. direction to Borthwick, and has upheaved at the same time, both the felspathic rock and the more ancient sandstones.

Borthwick hill is a vast mass of basaltic greenstone, which has been erupted through the old red sandstone strata at the place of intersection of several fissures which are likewise filled with trap. One of these already referred to, runs in a NNW. direction under the Knock hill, not however *directly under its centre*, but towards the west side, and has thereby thrown the mass of felspathic rock, of which that hill consists, *over to east*, producing a partial retroflexion of the sandstone strata on that side. In this respect, it exactly resembles the Cumledge trap-dyke, which, as formerly mentioned, has thrown the granitic mass of the Staneshiel hill also over to eastward. Another striking point of resemblance between these two dykes, is the occurrence of an insulated mass of *onion basalt*, exactly in the line of continuation of each. The basalt in the line of the Borthwick trap-dyke occurs (in the channel of the burn which falls into the Whiteadder at Elm Cottage) about 3 miles NNW. from the Knock hill. It consists of concretionary masses of various sizes, up to a foot or more in diameter, composed of nuclei of bluish grey basalt, coarse in the grain, and very hard and heavy, surrounded by concentric coats of a tufaceous substance. There is a slip in connection with the line of fissure, from which the felspathic rocks of the Knock hill, and afterwards the augitic traps now under consideration, were erupted. This has caused a downcast of the old red sandstone strata on the west side to an extent of 300 or 400 feet.

At Oxendean Commonhaugh, a mass of basalt is wrought in three quarries. It has been erupted through the old red sandstone, some of the strata of which are seen resting on its surface, much broken and altered by the heat. The original red of these beds has been changed to a dull purple, and the fragments into which they have been shivered are much indurated. The trap immediately subjacent is much debased by having absorbed, when in a molten state, a quantity of the detritus of the sandstone. The result is a curious compound, which is neither trap nor sandstone, but a mixture of both, and which graduates insensibly into the aqueous rock on the one hand, and the igneous on the other. In some places, also, the pulverized sandstone has got into fissures in the trap, where it has afterwards consolidated, presenting the singular phenomenon of *veins of sandstone in basalt*. The sandstone of these veins exhibits *vertical lamination*.

The Basalt of Dunselaw is similar to the above, with which it is connected. It has also been erupted through the old red sandstones, which rise towards it all along the south side, as shewn by excavations in the town of Dunse and neighbourhood. A large block of metamorphic red sandstone, which is known by the name of the "*Covenanters' Stone*," may be seen near the top of the hill. It seems to be a portion of a stratum which has been detached and borne up on the surface of the trap. It is hard and granular, and dips to ENE. at a high angle.

The *trap of the Castleknowes* runs from Dunselaw in an ENE. direction, and probably joins the Cumledge trap-dyke at right angles. At the New Tile Works, about a mile east from Dunse, it is a beautiful amygdaloid, containing nodules of various minerals, coated with green earth. On the face of the bank south of the Castleknowes, the old red sandstone strata are seen dipping away from the dyke, and divided by cleavages corresponding to those of the trap which are NNW. by ENE. Nearer the dyke they are much shattered, and are very hard and crystalline.

The trap of Grindean, is a basaltic ridge which runs in a SSE. direction, and appears to be connected with a line of fault similar to that of the Cumledge dyke, which runs parallel to it, the old red sandstone strata appearing on the *east side*, and those of the coal measures *on the west;* while the distance between the two formations is much too small to admit of the strata, occupying an intermediate space in the series, being brought on. The usual effects have been produced by the trap upon the adjoining sandstones. In some specimens the transition from a rubbly sandstone to a beautiful compact jasper is finely shewn.

A very interesting circumstance observable with regard to all the augitic traps in this district, is, that their course is either in a NNW. direction, or in one at right angles thereto, viz. from WSW. to ENE,—and these are invariably the directions of their cleavages. They likewise appear to be all connected together, forming one great system, indicative of one epoch of eruption. Thus the trap of Borthwick hill is connected with that of Oxendean Commonhaugh, while the latter joins that of Dunselaw, by means of a ridge running by St Mary's Cottage. The Castleknowes trap, again, joins the Cumledge trap-dyke to Dunselaw. Another large trap-dyke (a small portion of the course of which is laid down on the accompanying map), can be traced from Raecleughead hill by Langton, Grueldykes, &c., for several miles in an ENE. direction, and seems to join the prolongation of the Cumledge dyke in the neighbourhood of Edrom, about four miles from Dunse. A line drawn from the Castlemains hill in an ENE. direction, passes through the basaltic rocks of Anglemyheart, and meets the trap of Grindean. This line appears to be that of an extensive fault, the effects of which are manifest in the extraordinary disturbance of the old red sandstone strata, at the end of the Prestonhaugh section, near Anglemyheart. Between the latter place and Grindean, this dislocation (which is probably associated with a trap-dyke) appears to separate the old red sandstone and coal measures, the latter being thrown down on the south side; but, unfortunately, no section is exposed in the course of this line.

The consideration of the agencies which have influenced the directions of the trap-dykes and lines of fault within the district, and changed the positions and characters of the sedimentary rocks, is of great geological interest, but the principles involved are of too general a character to admit of being discussed in a memoir, descriptive merely of a small district. We therefore abstain from theorizing upon the facts adduced, the more important inferences which these suggest being for the most part sufficiently obvious. The views of the author on this branch of the subject, are also in part indicated on the Map and Sections, lithographed on Plate II.

VI.—*On the Extraction of pure Phosphoric Acid from Bones, and on a new and anomalous Phosphate of Magnesia.* *By* WILLIAM GREGORY, *Esq.*, *M.D.*, *F.R.S.E.*, *Professor of Chemistry in the University of Edinburgh.*

[Read 3d March 1845.]

I.—*On the Preparation of Pure Phosphoric Acid.*

THE usual methods of obtaining pure phosphoric acid by the oxidation, with nitric acid, or by combustion, of pure phosphorus, are well known; but, although they yield a pure product, yet, as the phosphorus must be prepared from phosphoric acid, it is obvious that we shall derive a great advantage from any method of purifying easily and cheaply the phosphoric acid from bones, instead of first reducing it to phosphorus, and then re-oxidizing it. In practice, phosphorus is made from the superphosphate of lime, and it is from the same salt that phosphoric acid may be most economically prepared.

Two processes, already given for this purpose, are worthy of notice. It is to be borne in mind, that the superphosphate of lime is the soluble compound obtained by acting on burnt bones with sulphuric acid and water, and filtering to separate the sulphate of lime.

In the first process, the solution of superphosphate is neutralized with ammonia or carbonate of ammonia, which precipitates all the lime in the solution, with about one-fourth of the phosphoric acid, as bone phosphate; while three-fourths of the acid are converted into phosphate of ammonia. The filtered liquid being evaporated, deposits crystals of that salt, which, when purified, are decomposed by heat in a platinum crucible; the ammonia and a great part of the water being expelled, while the phosphoric acid, with one equivalent of water, or metaphosphoric acid, is left. The objections to this process are the following:—The salt, when heated, melts, spirts much in boiling, becomes viscid, and froths up to a most inconvenient degree, requiring vessels of platinum of a large size for small quantities of material. Secondly, a very high and long continued heat is required to expel all the ammonia; and at that temperature, a portion of phosphorus is reduced by the hydrogen of the ammonia, and corrodes the platinum, leaving a blue stain of phosphuret of platinum. Besides this, the purest phosphate of ammonia often contains a trace of organic matter, which causes the glass of phosphoric acid, thus prepared, to be disfigured by carbonaceous particles. At least I have always seen black particles in the phosphoric acid made by this process. It is obvious, that if carbon be present, we have an additional source of reduced phosphorus; and if the black particles are phosphuret of platinum, then they shew that the platinum has been strongly corroded. In short, this method, save on a very small scale, is so objectionable, that it is seldom employed except for illustration.

The second process is one recently proposed, I believe, by LIEBIG, founded on the fact, that the whole lime may be removed from the superphosphate of lime, by the addition of sulphuric acid to the concentrated solution. This causes it to become quite thick from the large quantity of sulphate of lime produced. Cold water being added, the whole is filtered, and the filtered liquid and washings again concentrated and filtered from any sulphate of lime deposited during the evaporation. The concentrated liquid is again tested for lime by sulphuric acid, and if no change ensues, the lime has been entirely removed, as I ascertained by the proper tests. The solution now contains only the whole phosphoric acid of the bones, the magnesia which they always contain, and more or less free sulphuric acid. I have described this process thus far minutely, because, up to this point, it is the same as that which I recommend; and it is in the mode of separating the magnesia that the advantage of my process consists.

LIEBIG acts on the concentrated acid solution, brought to the consistence of syrup, by alcohol, which dissolves the phosphoric acid, leaving undissolved the greater part of the phosphate of magnesia, and depositing the last traces when allowed to stand. I cannot ascertain whether this operation is to be performed before or after the sulphuric acid has been expelled by heat; but I find, that after expelling the sulphuric acid, a transparent and colourless glass is obtained, which dissolves perfectly in boiling water, and the solution concentrated to a syrup, and treated with alcohol, yields a solution containing much magnesia, and which, on standing for weeks, deposits nothing. It is very probable that the alcohol did not succeed in separating the magnesia in my experiments, because the phosphoric acid was in some one of its modifications, different from that in which LIEBIG employed the same method. But I have not yet been able to manage that process so as to answer the purpose intended; and, even if it did succeed better, it is well known that alcohol, at its present price, cannot be used in this country on the large scale. I therefore endeavoured to find means of dispensing with its use, and I began by studying the properties of the soluble glass above mentioned, which contains only phosphoric acid, water, and magnesia.

This glass dissolves slowly, but perfectly, in boiling water; but the solution, when again concentrated, and so far deprived of water, that its temperature in an open platinum capsule rises to nearly 600°, suddenly becomes turbid, from the separation of a powder, while crystals begin to form in the viscid mass, resembling those which form in honey. When cold, water dissolves these crystals instantly, and leaves undissolved only a heavy white powder, which is a peculiar phosphate of magnesia. I shall return to it presently. It is quite insoluble in water.

As this salt was evidently formed at a certain temperature, I could see no reason why the whole of the magnesia might not be converted into that insoluble form, although, in this first experiment, the filtered liquid was found to contain much magnesia. I therefore again evaporated the filtered solution; and, at the same temperature as before, it again became turbid. I kept up the same heat for fifteen minutes; and when the mass, after cooling, was acted on by water, a large quantity of the insoluble phosphate separated, and the filtered liquid was now found absolutely free from the smallest trace of magnesia. It now clearly appeared that the first heating had been too short, and that it was only necessary to heat long enough to bring every part of the mass to the same temperature.

The process which I recommend, therefore, is as follows:—The glass remaining, after heating to redness in a covered platinum crucible to expel sulphuric acid (after the separation by that acid of all the lime), is to be boiled in water, and the solution evaporated, and, finally, exposed in a platinum capsule for a quarter of an hour, to a heat of from 595° to 600°, or to that temperature at which the acid begins to volatilize with the water. It must not be heated more strongly, lest the glass should be reproduced. When cold, the mass is to be softened with water, and the solution of pure phosphoric acid filtered from the insoluble phosphate of magnesia. The acid is pure from magnesia, if, when diluted and supersaturated with ammonia, it forms no deposit, especially after standing for one or more days. I have repeated the process six or seven different times, and on no one occasion did the filtered acid contain magnesia, except when I had purposely heated too strongly. This proves that the degree of heat is not difficult to manage. Indeed, after the first experiment I found it unnecessary to use the thermometer, the appearance of the mass furnishing a sufficient guide. It is evident that the above process has the twofold advantage of simplicity and economy.

The quantity of magnesia which separates in the form of the insoluble phosphate is very considerable. It will be very easy to determine exactly the amount of magnesia in bone-earth, by converting it into this salt.

II. *On a new and anomalous Phosphate of Magnesia.*

This is the salt so often mentioned above, as being separated by a heat of 600° from its solution in phosphoric acid. As the salt is perfectly anhydrous, it is obvious that its formation is owing to the separation of water at that high temperature. The analysis of the salt was made by fusing with carbonate of soda, dissolving in diluted hydrochloric acid, adding, for precaution's sake, a little phosphate of soda, and then precipitating by ammonia, collecting the ammoniaco-magnesian phosphate on a filter, washing moderately with cold water, drying and igniting. The residue, pyrohposphate of magnesia, was reckoned to contain

36.67 per cent. of magnesia. In three analyses, the per-centage of magnesia in the new salt was found to be 16.78, 16.92, and 15.94. In the last of these I pushed the washing further than in the first two, and I did the same in three more analyses, which yielded 15.12, 15.34, and 15.12, per cent. of magnesia. The mean of the six analyses, all made with portions of salt prepared at different times, is 15.87 per cent. of magnesia. As the salt lost no weight by ignition, it contained no water, and therefore was composed of magnesia and phosphoric acid alone: the absence of lime being previously ascertained. Its composition is therefore,

Magnesia, ...	15.87
Phosphoric acid, ...	84.13

The only formula which at all approaches to this composition is $3\ P_2\ O_5 + 2\ Mg\ O$, according to which, it is an acid sesquiphosphate of magnesia. The composition, calculated according to this formula, is,

Magnesia, ...	16.18
Phosphoric acid, ...	83.82

Considering the imperfection of the means for determining the amount of magnesia in analyses with precision, there can, I think, be no doubt, that the above formula expresses, empirically, the composition of the salt in question.

But what is the rational formula of this salt? As far as I know, there are no known sesquiphosphates of protoxides. Indeed, the very characteristic of the three known classes of phosphates, is their tendency to form salts with 1, 2, and 3 equivalents of base for 1 of phosphoric acid.

Metaphosphoric, or monobasic phosphoric acid, forms salts of the general formula, $P_2\ O_5,\ M\ O$; and if our salt is to be classed as a metaphosphate, it must be one with the very singular formula $2\ (P_2\ O_5,\ Mg\ O) + P\ \ O_5$; in which 2 eq. of metaphosphate of magnesia are combined with 1 eq. of anhydrous phosphoric acid, which has either entered the radical of the acid, without increasing its neutralising or assumed a neutral character.

Pyrophosphoric acid, or bibasic phosphoric acid, forms salts of the general formula, $P_2\ O_5, 2\ M O$, or $P_2\ O_5 \left\{ \begin{matrix} M\ O \\ H\ O \end{matrix} \right.$. If our salt be a pyrophosphate, and the temperature appears favourable to its being so, its formula must be $(P_2\ O_5,\ 2\ Mg\ O) + 2\ P_2\ O_5$, in which 1 eq. pyrophosphate of magnesia is combined with 2 eq. of anhydrous phosphoric acid, in one of the characters alluded to above.

Lastly, common or tribasic phosphoric acid forms salts of the general formula, $P_2\ O_5,\ 3\ M\ O$; $P_2\ O_5 \left\{ \begin{matrix} 2\ M\ O \\ H\ O \end{matrix} \right.$, or $P_2\ O_5 \left\{ \begin{matrix} M\ O \\ 2\ H\ O \end{matrix} \right.$. If our salt belong to this class, its formula will be $P_2\ O_5 \left\{ \begin{matrix} 2\ Mg\ O \\ P_2\ O_5 \end{matrix} \right. + P_2\ O_5$. Here, 1 eq. of phosphoric acid, acting as acid, is united to 2 eq. of magnesia, and 1 eq. of phosphoric acid, acting as base, while a third eq. of phosphoric acid acts in another capacity, possibly as a neutral body, like water of crystallization.

In all these supposed formulæ, we have anhydrous phosphoric acid acting in an unusual capacity; and it is evident that, whichever we adopt, the occurrence of the salt is favourable to the doctrine, that the so-called anhydrous acids are not really acids. It is true that, on the old view of phosphoric acid and phosphates, there is nothing startling in a sesqui-phosphate; but, if we adopt this view, we must cast aside all the knowledge recently acquired concerning the phosphates, and which, to a great extent, is established by experiment.

On the whole, the composition of this salt is so anomalous, that we must conclude, either that the received views on the subject of the phosphates are erroneous, or that there exists a fourth modification of phosphoric acid, distinct from the three usually admitted. We might suppose this acid to be $3\,P_2\,0_5 = P_6\,0_{15}$, and to be neutralised by 2 eq. base, yielding the formula $P_6\,0_{15}, 2\,M\,0$.

It is worth while to remark, that, on the theory of compound acid radicals and hydrogen acids, according to which the formula of a tribasic phosphate is $P_2\,0_8, M_3$, that of a bibasic phosphate, $P_2\,0_7, M_2$, and that of a monobasic phosphate, $P_2\,0_6$, M, this salt cannot be represented as a sesqui-metaphosphate, although, as we have seen, it may be so on the old view of acids and salts. The latter makes it $3\,P_2\,0_5 + 2\,Mg\,0$; but the former would require 1 eq. of oxygen more to yield the formula of a sesqui-metaphosphate $3\,P_2\,0_6, Mg_2$, the salt containing only $\left.\begin{matrix}2\,P_2\,0_6\\ P_2\,0_5\end{matrix}\right\}Mg_2$. In this point of view, the existence of the new salt may turn out to be a serious objection to the above named theory of salts and acids.

The new phosphate is remarkable, among all the salts of magnesia, for its extreme insolubility in water and acids, which is such, that it may be ranked beside sulphate of baryta. This insolubility has hitherto defeated all my efforts to ascertain the precise nature of the acid it contains, and whether that acid be new or not.

As this salt is not only very insoluble, but easily washed, and less hygrometric than any powder I have ever weighed, it would be well adapted for the determination of magnesia, and also of phosphoric acid, if we could, at pleasure, convert these substances into this particular form of combination when mixed with other bodies. The experiments I have made on this subject have convinced me, that there are very great difficulties in the way of this application; but, I trust, they will not be found insuperable. It will, at all events, be easy to determine the amount of magnesia in bones, by converting the magnesia, as in the above process, into this insoluble form.

VII.—*Miscellaneous Observations on Blood and Milk.* By JOHN DAVY, *M.D., F.R.S., Lond. and Edin., Inspector-General of Army Hospitals, L.R*

(Read April 7. 1845.)

1. *On the State of Combination of the Alkali in the Blood.*

The condition of the alkali in the blood—of that portion on which its alkaline reaction depends—has been the subject of much speculation, and of many experiments. ENDERLING is one of the latest inquirers who has given it his attention. After having made an analysis of the ashes of the blood, he has come to the conclusion, that the alkali in it is in combination with phosphoric acid, the former predominating in the form of the tribasic phosphate of soda.*

Granting the accuracy of ENDERLING's analytical results on the ashes, does it follow that his inference must be correct relative to the condition of the alkali in the liquid blood? It appeared to me doubtful *a priori;* and the doubt I entertained was confirmed by experiment. The doubt arose from considering the tendency of the alkaline carbonates, when strongly heated with charcoal, to be reduced; and when heated with phosphate of lime in excess, to exchange their carbonic acid for a portion of the phosphoric—the acid gas of course escaping, and compounds of lime and alkali remaining, each with excess of base. In accordance with this, when I have added carbonated alkali to the coal obtained from blood, and have reduced the coal to ashes, I have not been able to detect in the lixivium obtained from them any trace of carbonic acid. Moreover, I find that the carbonate of soda is liable to loss when heated strongly, exposed to the air; and, consequently, when it exists in a small quantity in a bulky coal, the whole of it may be dissipated—carried over much in the same manner as boracic acid is in combination with water as a hydrate, when it is subjected to heat.

If ENDERLING's view were correct, the blood, after having been acted on by the air-pump, ought not in its fresh state to yield any carbonic acid on the addition of an acid. This is the experiment alluded to, which confirmed my doubt. I find that blood or its serum, after having been so acted on until perfectly tranquil, has effervesced strongly, when mixed either with dilute sulphuric or muriatic acid purged of air, or with a solution of cream of tartar. And, in accordance with this, I have also found that serum, after having been subjected to the air-pump, gives on coagulation, by immersion in boiling water, a different result, whether immersed unmixed, or after admixture with a little acid. In the one instance no air bubbles are disengaged; in the other very many.

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* See Mr PAGET's Report on the Progress of Human Anatomy and Physiology, in the British and Foreign Medical Review for January 1845.

Some years ago, when engaged in experiments on the blood, especially in relation to the present question—the condition of the alkali in it—I noticed the effect of cream of tartar in expelling carbonic acid, and that both from venous and arterial blood, and from serum; an effect which, with other considerations, induced me then to conclude, that the soda in the blood exists in the form of the sesqui-carbonate; an inference which appears to me still to be most in harmony with the facts.*

In opposition to this view, perhaps, it may be said, that farther proof of its correctness ought to be afforded by the effect of a solution of muriate of lime on the serum,—that, if the latter contain the alkali as stated, a precipitate of carbonate of lime ought to be the result. This experiment I have tried, with the aid of the air-pump, sometimes with a doubtful result, sometimes with a negative one, especially in the instance of serum from venous blood. But in these instances I have also found the result the same, even on the addition of a portion of sesqui-carbonate of soda, as much as ·2 of a grain to 316 grs. of serum—a quantity of the alkali, which, when dissolved in the same bulk of water, is more than sufficient to give a precipitate with muriate of lime. Would not this seem to indicate that in the blood and its serum the carbonated alkali is in a peculiar state of combination with the animal matter; and the same remark is applicable to the posphates or their elements.

The trials referred to have been made on the blood and serum of the ox and sheep, at a favourable time of the year, during the winter season, when the temperature of the air has been little above the freezing point.

2. *On the Viscid Quality of the Blood Corpuscles.*

That the corpuscles of the venous blood of the mammalia, when quite fresh, and in the act of coagulating, collect together in piles, as it were by a kind of attraction, is well known. The viscid, adhesive quality, I am about to notice, is distinct from this, and, indeed, is best seen when the aggregation in piles ceases to be witnessed, as in cruor, procured by breaking up the crassamentum, and separating the fibrin by straining through linen.

The cruor thus obtained is essentially a semifluid, the particles loosely adhering forming a mass in some respects not unlike honey or molasses. I shall notice some appearances connected with and indicating the condition referred to.

When poured into a fluid, such as water or serum, it rapidly falls to the bottom; and, in the instance of serum, if not agitated, remains as a connected mass. If now a glass rod be put into it, and withdrawn through the supernatant serum, it will come out not sensibly coloured by the red particles; the surface of the cruor round the rod will be seen to be raised a little in the act from adhering to it, and then to return to its former level, shewing that the corpuscles adhered to each other in the mass more strongly than to the glass: and, if the serum through which the rod has been drawn is examined with the microscope, a small number only of blood corpuscles will be detected in it.

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* Physiological and Anatomical Researches, ii. p. 152.

If, instead of allowing the cruor to remain undisturbed, it be broken up by agitation with the serum, it will be found to be divided into clusters of corpuscles and detached particles. When one of these clusters is placed under the microscope, between two plates of glass, the adhering corpuscles forming the group are seen to be attached, not by their broad or concave surfaces, as in the instance of aggregation by piles, but by their narrow rims. Now, if graduated pressure be employed, so as to break up the cluster, just before separating, the adhering corpuscles will be seen to be elongated, as if drawn out almost to a fibre, and yet when detached, the adhesion being overcome, recovering, and that suddenly, their circular form: and, on relaxing the pressure, many of them will be seen to reunite, sticking to each other even when in motion.

This adhesive quality of the blood corpuscles is exercised, not only on each other, but also on other substances, though, perhaps, in a less degree. Proof of this is afforded when cruor has been allowed to remain, even but a short time, in a glass tube, or any other vessel. The portion in contact with the bottom of the tube is found to adhere to it, and is not easily detached; whilst any that may adhere to the sides commonly appears in streaks, the blood corpuscles being attached to each other, and so producing a linear arrangement.

This viscid property of the blood corpuscles must, I apprehend, be considered as specially belonging to them, quite distinct from the fibrin, which appears to be viscid only in its transition state, in the act of coagulating,—previously even more liquid than serum attenuating the blood, and subsequently, as soon as coagulated, constituting the firmest and the cementing part of the crassamentum. The blood corpuscles, as regards this quality of viscidity, are far more constant: it belongs to them when fresh, probably when circulating in the vessels,—it is exhibited in them long after removal from the living body, and is not even lost with incipient putrefaction, and, connected with that, the change of the particles to a globular form.

3. *On the Tendency of Fibrin in Coagulating to a certain arrangement of its Particles.*

Amongst the many remarkable properties of coagulable lymph, I am not aware that a tendency of its particles to arrange themselves in a certain manner out of the body, representing, as it were, what takes place in the body in the process of growth and of reparation, has hitherto come under observation, or, at least, has been the subject of commentary.

A striking instance of the kind I have witnessed in the buffy coat. When the buffy coat is well marked, as in cases of acute rheumatism, when it is thick and cupped, the blood abstracted having been slow in coagulating, it is easily detached from the soft crassamentum; and this is best done under water. Thus separated, it may be described as a fibrous mass loaded with serum, enveloped in a pellicle or membrane, performing the part of a sac. This pellicle, or containing membrane, is very thin, yet of considerable strength, and with care may be dissected off, especially after maceration in water for two or three days, at a low temperature. It is very like a serous membrane, both as seen with the naked eye, and under the microscope. Under the latter, it bears a strong resemblance to the arachnoid, appears as a tissue of extreme delicacy; hyoloid, without any visible pores or fibres, with a few particles like blood corpuscles, or their remains (according to the method used of separating it), scattered through it. When a force is applied to it, it breaks less readily in one direction than another; and exhibits, when drawn in one direction, more elasticity than in the opposite. When the blood, as is usual, has been received in a circular vessel, and the buffy coat, of course, is of the same form, tearing the membrane with a forceps towards the margin, shreds of it, several lines in length, are easily detached in a line from the centre to the circumference, but not in a line at right angles to this; and in the same direction small portions of the membrane exhibit considerable elasticity, which they do not in the opposite direction.

I may mention another example, also well marked. If the blood, in the act of coagulating, is stirred with a glass rod, or a wooden skewer, or the like, the fibrin, as it is well known, will adhere, with which blood corpuscles will be mixed. The adhering clot, consisting of the two, the fibrin in excess, when pulled off, which it easily is, exhibits a canal with a smooth inner surface. If it be well washed to deprive it of colouring matter, and slit open, it will be found to bear a close resemblance to an artery, especially to its middle coat, being composed of fibres arranged seemingly transversely, that is, at right angles to the axis of the tube. This is to be inferred from the effect of a force applied. If applied in that direction, transverse shreds pretty readily separate; but if in the opposite direction, using a forceps, only small bits. And, in the one, the transverse direction, the tube is far more elastic than the other, after the manner of the middle arterial coat.

Other instances might be given, tending to shew the same disposition on the part of coagulable lymph to a certain regular arrangement of its parts, as it were, of a *nisus formativus*, in the act of coagulation. In examining the buffy coat, or the fibrinous masses which are so commonly met with after death in the right cavities of the heart, it is not uncommon to find in them, when divided, cavities containing serum resembling cysts. And in the ventricles of the heart, and the aorta and the principal veins, especially the iliac and femoral, fibrinous concretions, as it is well known, are often found after death from lingering diseases, in which a puriloid matter is contained, as in a sac,—a matter which has been imitated by Mr GULLIVER, by the coction of lymph, at about the temperature of the human body, and which, previous to his experiments, had been considered as pus, and, erroneously, as the product of inflammation.

I would ask in conclusion, is not this disposition of coagulable lymph called into play in other occasions during life, and may it not serve to explain certain appearances which are commonly accounted for in a different manner, such as the cysts which so rapidly form in the instance of aneurisms, the consequence of wounds, and the lining membrane of the sacs of false aneurisms, which is hardly in appearance distinguishable from the inner coat of the artery with which it is continuous?

4. *On the Effect of Serum in promoting the Coagulation of Milk.*

There is a marked difference, as is well known, between the albuminous part of the serum of the blood and that of milk,—ordinary cow's milk,—viz., that, whilst the former is coagulated by a temperature below the boiling point of water, the latter, in its fresh state, is not so affected, even by ebullition, but, on the contrary, has its natural tendency to coagulate, connected with the absorption of oxygen and the formation of an acid, retarded. *A priori*, perhaps, it would harldly be expected, as regards the property of coagulation, that the one fluid mixed with the other would have any material effect. But that it is not so, I have found on trial. Milk, I find, when mixed with serum in certain proportions, is coagulated by heat. I shall notice some results obtained, using mixtures of the serum of the blood of the sheep, which coagulated at about 170° Fah., and cow's milk.

Equal parts of the two remained liquid at 170°, and coagulated about 175°. The coagulum was of an opaque white, very little softer than the coagulum of the serum alone. Mixed with water it did not render it milky; and the watery infusion was not rendered turbid by acetic acid, and only in a very slight degree by the nitric acid.

One part of serum and three parts of milk in mixture, coagulated at about 190°, forming a soft tremulous mass, which, by boiling, was rendered firmer. Broken up and mixed with water and filtered, the fluid had the properties of weak whey, and was not rendered turbid by acetic acid.

A mixture of 1 part of serum and 4 of milk did not coagulate after several minutes boiling; but, keeping it on the fire, in about a quarter of an hour the effect was produced. The coagulum formed was very soft. Mixed with water, it rendered the water turbid at first; but after a while, the finer particles subsiding, left the water clear, and it was not affected by acetic acid.

A mixture of 1 part of serum and 5 of milk, immersed in boiling water, at least an hour, was found coagulated. The coagulum was of the consistence of very soft custard. A like result was obtained, using a mixture of 1 of serum and 7 parts of milk.

Lastly, a mixture of 1 part of serum and 10 of milk was liquid after more than an hour's boiling; but after about three hours' boiling, when a portion of its water was expelled, it was found coagulated.

As in these instances, the serum promotes the coagulation of the milk, so also the latter may be considered as favouring the coagulation of the former; that is, viewing the milk as a diluent of the serum, having the effect of removing further apart its albuminous particles, and comparing it as a diluent with water; two parts of which, I find, with one of serum, prevent the coagulation of the latter, even at the boiling point, and thus heated, for many minutes.

The property exhibited in the foregoing experiments is to be witnessed in many other instances; it may be, probably, in every instance in which liquid casein and albumen are heated together in certain proportions. Mixtures of white of egg and milk exhibit it even more strongly than serum and milk. With equal parts of milk and white of egg, a firm coagulum is formed on boiling, the fluid exuding from which is almost transparent, and is not sensibly affected either by acetic acid or by the nitric acid. With 1 of white of egg and 5 of milk a coagulum is formed of moderate consistence; and with 1 of the former and 10 of the latter a very soft coagulum, after immersion in boiling water for about a quarter of an hour. I give these results, because, in no work on chemistry, with which I am acquainted, have I met with any account of the effect of white of egg on milk, even in culinary processes. Using the yolk of the egg in place of the white, the effect on milk has been, as might be expected, very similar; the chief difference in the coagulum being, that it has been somewhat softer.

From similarity of composition, it might be inferred that the roe of fish, mixed and boiled with milk, would have the effect of coagulating it; and this, I find, is the case. The only other animal substance I have tried has been muscle; it, triturated and boiled with milk, did not coagulate it; the muscular fibres, it may be noticed, were found collected together in a mass in a singular manner.

Extending the analogy to vegetable substances, it seemed likely that all those which contain albuminous matter, similar to casein, may be affected when boiled with serum or white of egg, in the same manner as milk; and the result of a trial, with a mixture of serum and a strong emulsion of sweet almonds, has confirmed the inference. After boiling, the fluid expressed from the coagulum was not precipitated by acetic acid.

As the serum of the blood of even the same species of animal is liable to slight variations, affecting the degree of temperature at which it coagulates, and, as milk is subject to some variation, in regard to the same quality, as indicated by the effect of rennet—a variation, perhaps, most of all depending on season of the year, and the time that the milk has been exposed to the atmosphere—should the experiments I have described be repeated, some little difference in the results may perhaps be perceived, depending on the circumstances just referred to.

The action of one animal fluid on another, and those so similar as regards their albuminous part, as milk and serum, offers curious matter for speculation, and may be deserving of special attention, not only in relation to the culinary art, and processes of manufacture in which vegetable juices are concerned, but also in connexion with physiology. It may be found that the principle of rennet exists in the blood, and that the analogous power of both, as regards the coagulation of milk, depends on the same cause; and, if so, then it may be farther deserving of attention, in connexion with pathology and processes of morbid softening.

THE OAKS, AMBLESIDE,
February 17. 1845.

VIII.—*On the Advantages to be derived from the Use of Metallic Reflectors for Sextants and other Reflecting Instruments; and on Methods of directly determining the Errors in Mirrors and Sun-Shades used in Reflecting Instruments.* By JOHN ADIE, Esq.

(Read February 17. 1845.)

It has frequently occurred to me that the difficulty experienced by instrument-makers, in obtaining for sextants and other similar instruments, reflecting mirrors perfectly parallel in their polished surfaces, and also the greater difficulty of procuring glass perfectly homogeneous in its structure, might be overcome by the use of metallic reflectors.

It is well known, that, from the want of perfect parallelism in glass mirrors, there arises an error in the reading of such instruments, inasmuch as the emergent ray does not pass out of the glass at the same angle as the incident falls upon it, and that from the want of homogeneousness in the substance, and the unequal refractions caused by the veined structure of the glass.

Whether this structure of the glass arises from the process of its manufacture, or the want of proper admixture of the component parts, before being cast into plates, I am not prepared to say; but in all the plate-glass I have tried, by polishing it on the edges, this structure was observed; so much so, that the plates, on being seen through perpendicularly to the plane of their surface, shewed objects perfectly distinct; while objects, when viewed through the glass at right angles to this plane, were seen with difficulty, distorted and twisted in all directions. Of such glass, the mirrors of sextants, and other reflecting instruments, are made; and it is easy to conceive how very erroneous the angles may be, particularly when the incident ray falls on the mirror at a low angle, as it does when large angles are observed, as in lunar distances and the like; while the indistinctness of the image observed under these circumstances, detracts much from the utility of instruments fitted with such mirrors. As a practical illustration of the above, if we take a number of objects, and observe with a sextant the angles between each, then observe the angle between the extremes, suppose this 120°, it will be found, in the great majority of cases, that the sum of the angles observed does not agree with the observed angle of the extremes, which should be the case.

These errors, and sources of error, are obviated when we make use of metallic reflectors, having their surfaces polished perfectly flat; a matter of no very difficult attainment in practice.

But, besides the avoidance of error, there are direct advantages in the use of such reflectors, which may be stated thus:—In the marine sextant, or reflecting circle, the reflection of faint objects is more easily obtained; in other words, objects are seen reflected by metallic mirrors which cannot be seen by the ordinary silvered ones. Another advantage is, that larger angles can be observed. This applies more particularly to the pocket or box-sextant, used in surveying, both at sea and land. From the small size of the index mirror, we cannot, when glass is used, reflect an angle much above 100°, the thickness of the glass cutting off the incident and emergent rays, when these fall on the silvered surface at low angles; whereas, with the metallic reflector, the reflected angle can be obtained to its utmost limit, or to about 140°, being nearly one-half greater than that which can be obtained by means of a silvered glass reflector.

I am not aware of any account having appeared of the use of metallic mirrors heretofore in the construction of such instruments, although I have little doubt, from the obvious advantages attending them, that the idea must have suggested itself to many others; and that the fact of their not having been brought into use, must be accounted for from the difficulty of obtaining speculum metal possessed of the requisite qualities.

The liability of a highly-polished reflecting surface to be destroyed by tarnish and rust, from exposure to the atmosphere, and more particularly from exposure to the influence of sea air, is an objection that occurs on first view to the use of metal.

All who are acquainted with the reflecting telescope, know how subject the mirrors of such an instrument are to deterioration from tarnish; and that, in many cases, even when due care has been taken of them, they have been altogether destroyed. Yet, it should be stated, that this is not the case with all such instruments. There are many reflecting telescopes, now very old, in which the mirrors are in a state of perfect preservation; those I am best acquainted with, as having stood the test of time (and they are in general very good), having been made by the late James Short of London, who lived about eighty years ago. On the other hand, it is well known, that many speculum metals will not retain their lustre for many weeks under ordinary exposure.

From these facts, we may infer, that it is the composition of the metal which causes the difference in the permanency of the polish.

My attention was therefore directed to procuring pure metals to form the alloy or speculum metal. Tin is not difficult to be had in a state of great purity; but it is otherwise with copper; for, as we advance in commerce, we find, that, day after day, this metal is brought to market more and more impure; so much so, that bar and cake copper of commerce are now so bad that they are nearly unfit for compounding as brass.

The recently discovered process of electrotype, however, affords us the means of easily procuring copper in a state of purity; and it is with the metal so procured my experiments have been made.

By compounding copper and tin in their atomic proportions of 16 parts of copper to 14.92 parts tin, a metal of high lustre is obtained; and, so far as my experiments have gone, this metal is not liable to tarnish, if ordinary care be taken to guard against this effect. My course of procedure was as follows:—I first exposed polished pieces of this metal to the free open air, and found, after some months' exposure, that, when the dust and rain stains were rubbed off, the surface was in a very good state of preservation. I next tried exposing the mirrors to the fumes of acids, and watering them with sea water for a considerable length of time. Under the operation of these corrosive agents, they still retained their lustre. But, notwithstanding the encouragement held out from these experiments, feeling yet reluctant to put such instruments into the hands of navigators without first submitting them to the test of actual service; and a favourable opportunity having last spring presented itself, through the kindness of Mr O. MOSSMAN, one of the surveyors on board of H.M. Ketch, Sparrow, then about to engage in a survey of the Pentland Firth, I put into the hands of that gentleman a sextant fitted with these metallic reflectors, of which he politely took charge, promising to give it a fair trial during a season's survey. Mr MOSSMAN amply redeemed his promise; and I shall now take the liberty of quoting the letters which he was good enough to address to me, giving an account of the working of the instrument.

"H.M. KETCH, SPARROW,
THURSO, 26*th October* 1844.

"DEAR SIR,—After you have examined the reflectors you will be able to judge of the durability of them, after being in constant use for most part of the season, during which, they have been exposed to all sorts of weather. As regards the power of their reflection (although dark), they are beyond all the silvered glasses I have ever had in use; also, for measuring large or small angles, they excel the others by far. When we are once properly settled at Portsmouth, I shall be able to make a full descant of the good properties of your metallic reflectors, and shall strongly recommend them to be in all reflecting instruments. I have subjected them to all exposure which I could call fair play, only, at the same time, having been careful not to put the instrument away damp.—Yours truly, (Signed) "WM. O. MOSSMAN."

In a second letter, dated Portsmouth, 13th December, Mr MOSSMAN says:—"I am sorry my spare moments are rather scarce, or I should have said something more concerning the merits of your metallic mirrors. However, I should very strongly recommend them to be used in all instruments that are likely to be exposed to much wet; because, if the instrument fall overboard, for instance, and does not lie so long in the water as to allow the rust to commence, if it be carefully wiped dry, when taken out, there is no danger of spoiling afterwards. Now, this is a very important matter, and more particularly to surveyors, than any other class of nautical men; for such accidents frequently occur in boat-soundings."

I come now briefly to notice the second subject announced in the title of this paper, viz., direct methods of determining the errors in the mirrors and sun shades used in reflecting instruments. I am not aware of any method adopted by practical men for the discovery of such errors, except that of a careful process of what is termed parallel grinding, and testing the glasses in the instrument when fitted up. This is effected by observing known angles, and noting that a contact of the sun's limb, by reflection, does not vary on changing the sun shades interposedbetween the direct and reflected images seen in the telescope. This variation is noticed by Mr M'Kay, in his work on Determining Longitude, and he recommends that it should be observed and applied as an index error affecting the several shades. Errors may, however, exist in the reflectors and shades, which, from the particular position they have in their settings, are not discovered by such trials. The methods I have adopted are as follows:—

First, For the mirrors I place a mirror at about an angle of 45° before the object-glass of a telescope, mounted on a divided circle, capable of reading an angle of 10″; I have, besides, a moveable micrometer wire at the stop holding the cross wires of the telescope, by which an angle of one second may be observed; the mirror placed before the object-glass rests on three smooth studs, to which it is pressed up by a light spring at the back. In this position we turn the telescope and mirror in azimuth till we obtain the reflection of a well-defined distant object, which is brought to the intersection of the cross wires of the telescope. If we now turn round, or reverse the mirror on these studs, and find the same object in the intersection of the cross wires, we know that in that line there is no want of parallelism; and if we have the same result on repeating the trial at right angles to the first direction, the mirror is said to be perfect in respect to the parallelism of its surfaces. If, however, we find on turning the mirror that the reflected object is not intersected by the cross wires, then, the glass is not parallel; and half the amount of error read by the circle or micrometer head, is the error which would arise if such a mirror were applied to a sextant.

Second, For the sun shades I have a telescope whose object-glass is divided, and the one-half moved over the other by means of micrometer screws, having a divided head. The value of the divisions of the micrometer head is obtained by measuring the sun's diameter. In that which I use, two divisions on the head are equal to one second.

Before one-half of the object-glass is placed a fixed sun shade; and before the other, an arrangement is made for placing the shades to be tested in such a way that they can be turned round. Having placed in this frame a shade for trial, I bring the two images of the sun seen in the telescope into contact, by means of the micrometer screws; and if, on turning the shade before the object-glass, the contact remains perfect, then we know that its surfaces are parallel; if, on the other hand, the contact is broken, we can, by turning the shade, find the point of nearest contact; and by turning the shade through 180°, we measure the amount of opening of the images, by means of the divided micrometer head. This gives double the error which would be caused by the shade when used in making a direct observation. But as all angles observed by reflecting instruments are double, or, in other words, the divisions on the limbs of reflecting instruments are equal only to half those of an instrument used for direct observation, the double error given by this method goes all to deteriorate the observation, as an unparallel shade affects the angle when reflected by twice the amount of direct observation. Another important object is gained by this method of testing the shades. It being a very difficult matter to obtain these altogether perfect, I reject all where the error would amount to 10″, which is the usual reading of a sextant; and when an error of a smaller amount does exist, I find, by turning the shade, the line in which the want of parallelism exists; and by placing this line at right angles to the plane of observation in a reflecting instrument, the angles observed by such an instrument are not affected even by the small error of the shade.

IX.—*On the Balance Magnetometer, and its Temperature Corrections* *By* J. A. Broun, *Esq.* *Communicated by* Sir T. M. Brisbane, *Bart.*

(Read 21st April 1845.)

1. The Balance Magnetometer was imagined by Dr H. Lloyd, of Dublin, for the purpose of observing the variations of the vertical component of the earth's magnetic intensity. It consists simply of a balanced magnetic needle, with a knife-edged axle, resting on agate planes, at right angles to the plane of the magnetic meridian. In the instrument from which the results in this paper are deduced, the position of the needle is observed by means of micrometer microscopes.*

2. If m be the moment of free magnetism of the needle, Y the vertical component of the earth's magnetic force, W the weight of the needle, g the distance of the centre of gravity from the centre of motion, ϵ the angle contained by the line joining these two centres, and the magnetic axis of the needle when horizontal; the equation of equilibrium will evidently be

$$m\,\mathrm{Y} = \mathrm{W}\,g \cos \epsilon \quad . \; . \; . \; . \; . \; . \; . \; . \quad (1.)$$

By differentiation and division

$$\frac{\Delta \mathrm{Y}}{\mathrm{Y}} = \tan \epsilon \,\Delta \epsilon - \frac{\Delta m}{m} \quad . \; . \; . \; . \; . \; . \quad (2.)$$

The differences $\Delta \epsilon$ are obtained by means of the micrometers, and the differences of Y in terms of Y will be obtained, if we can determine ϵ and $\frac{\Delta m}{m}$, the latter being the variation of the magnetic moment, due to temperature.

3. There are great practical difficulties in the way of rendering the needle capable of giving ϵ accurately by inversion, but Dr Lloyd has shewn† that

$$\tan \epsilon = \cot^2 \theta \frac{\mathrm{T}'^2}{\mathrm{T}^2} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (3.)$$

where θ is the magnetic dip, T′ and T the times of one vibration of the balance needle in a horizontal and in a vertical plane. We have thus, instead of one, three unknown quantities to determine; and it becomes a matter of importance to shew with what degree of accuracy this may be done.

The dip and time of vibration in a horizontal plane can be obtained with sufficient truth for the purposes of this factor.

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* See the Introduction to the Makerstoun Magnetical Observations for 1841-2.

† In his "Account of the Magnetical Observatory of Dublin," where the complete investigation will be found.

Observations of the time of vibration in a horizontal plane ranging through a period of three years, agree within $0\overset{s}{.}06$, and this difference must be to a considerable extent due to alterations in the condition of the needle between the different observations.

4. The time of vibration in a vertical plane is in widely different circumstances.

These differences I shall proceed to point out.

1*st*, The time of vibration in a vertical plane is found increased *after* the needle has been, by any means, vibrated through a large arc.

The strongest evidences of this are contained in the following table; they were obtained either by iron having been brought accidentally near to the magnet, or by the necessary removal of the box which covers it. In the latter case, the magnet was vibrated through large arcs by currents of air.

One or two observations for the time of vibration are given for the periods immediately before and after the disturbance of the magnet.

The last column contains the times of vibration corrected to 50° Fahr.; for reasons that will be shewn, it is only these that are strictly comparable.

The observed time of vibration is generally the mean of two series, which rarely differ one-tenth of a second.

Additional evidence of the above conclusionis furnished by Table II.

TABLE I.

Observations for the Time of Vibration of the Balance Needle in a Vertical Plane, before and after excessive Vibrations.

DATE.	CAUSE OF DISTURBANCE.	Observed Time of one Vibration.	Temperature of Needle.	Time of one Vibration corrected to 50°.
1842.		s.	°	s.
March 19.		10.14	45.5	10.48
... 22.	Balance magnet vibrated excessively.			
April 2.		11.02	42.5	11.59
... 9.		10.94	44.0	11.40
Oct. 31.		10.05	55.9	9.60
Nov. 10.	A pair of compasses brought inadvertently near the balance needle.			
... 11.		10.34	51.0	10.26
... 19.		10.02	50.8	9.96
1843.				
Sept. 18.		10.60	65.0	9.46
... 22.		10.62	63.7	9.58
... 26.	The box of the magnetometer removed, and the needle exposed to currents of air.			
... 27.		11.20	51.9	11.06
... 29.		10.95	46.1	11.40
1844. h				
April 29 22		9.04	50.2	9.02
... 30 7		9.50	60.5	8.70
... 30 7+	Box lifted for the purpose of removing an insect, and replaced immediately afterwards; vibration not excessive.			
... 30 8		10.03	60.5	9.23
... 30 22		8.97	50.5	8.93
July 22 0		8.23	62.3	7.30
... 23 22		9.10	68.0	7.73
... 24 18	Box lifted, and insect removed from beside the needle.			
... 25 0		9.80	67.2	8.49
... 25 23		9.67	64.5	8.57
... 26 3	The magnet vibrated by steel.			
... 26 23		10.04	65.4	8.87
... 29 22		9.45	58.5	8.80
Oct. 30 22		6.97	47.9	7.13
Nov. 3 22		6.72	43.9	7.18
... 3^d—7^d	Workmen in observatory, who had probably been near the magnet with a hammer.			
... 7 23		7.30	42.5	7.87
... 8 21	Workmen brought a hammer near the magnet. After the vibration thus produced, the needle rested in a position differing 1′.6 from its previous position.*			
... 9 1		8.24	46.8	8.48
... 10 23		8.03	43.7	8.51

5. *2d*, The time of vibration in a vertical plane depends, to a considerable extent, on the magnitude of the arc of vibration.

3d, For the same arc, the time of vibration is greater, if it belong to a series commencing with a large arc, than if it belong to a series commencing with a small one.

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* This is the only case in which I determined, at the instant, the effect of excessive vibration on the position of the needle; the effect, though small, is considerable, when compared with the hourly changes; for several hours before this vibration, the magnet had not changed its position.

These conclusions I had arrived at nearly two years ago, and accordingly only small arcs were used in determining the time of vibration, seldom above 5′.0 commencing.

The following series of observations was made in January 1844, before removing the needle for the purpose of determining its temperature correction. Many other series made previously give the same result; but the following will be sufficient to prove the facts stated above.

TABLE II.

Observations for the Time of Vibration of the Balance Needle in the Vertical Plane, for different Arcs.

Time at the commencement of each Series.	Semi-arc of Vibration.		Number of Vibrations.	Means.	
	Beginning.	Ending.		Partial.	Of the Series.
d h m	′	′			
Jan. 26 22 15	1.8	0.4	14		9.58
22 25	1.4	0.4	14		9.58
22 40	25.0	18.6	6	11.15	
	18.6	6.5	6	10.98	10.95
	6.5	0.5	8	10.71	
22 53	1.3	0.4	16		9.78
23 20	1.6	0.4	16		9.70
23 32	45.5	25.0	6	11.36	
	25.0	11.3	8	11.20	
	11.3	6.5	6	11.07	11.07
	6.5	2.1	6	10.95	
	2.1	0.5	6	10.78	
23 50	1.7	0.4	14		10.17
Jan. 27 0 5	55.0	40.0	6	11.72	
	40.0	30.0	6	11.60	
	30.0	22.0	6	11.57	
	22.0	17.0	6	11.45	
	17.0	12.3	6	11.37	11.35
	12.3	5.8	6	11.33	
	5.8	5.5	6	11.22	
	5.5	4.5	6	11.09	
	4.5	0.7	6	10.80	
0 20	1.7	0.4	18		10.60

The semi-arcs were observed by my assistant Mr WELSH, at one microscope, while the times of each vibration were observed by myself at the other.

It is not my intention, in the present communication, to enter into any examination of the causes of these peculiarities; my object is simply to point them out as sources of error. I shall therefore merely state my conclusions, with their evidences.

6. *4th,* The time of vibration in a vertical plane depends, to a considerable extent, on the temperature of the needle.

The following short series, taken at random from a great number of observations, at once prove the truth of this conclusion. From a comparison of a few of the observations, it was found that an increase of 1° Fahr. was equivalent to an increase of $0^{\mathrm{s}}.076$ in the time of vibration.

The last column for each series gives the times corrected by this quantity to 50° Fahr. That the correction obtained is only approximate, will, together with errors of observation, account for much of the discrepancies in the corrected quantities.

TABLE III.

Observations for the Time of Vibration of the Balance Needle in the Vertical Plane at different Temperatures.

Time of Observation.	Observed time of one Vibration.	Temperature of the Magnet.	Time of one Vibration corrected to 50° Fahr.	Time of Observation.	Observed time of one Vibration.	Temperature of the Magnet.	Time of one Vibration corrected to 50° Fahr.
1844.				1845.			
d h	s	°	s	d h	s	°	s
Jan. 2 22	8.91	31.4	10.32	Jan. 20 23	7.21	35.4	8.32
3 2	9.48	40.0	10.24	23 23	8.38	46.3	8.66
3 4	9.80	43.5	10.29	26 22	7.42	35.9	8.49
3 5	9.96	45.2	10.32	30 2	6.72	27.0	8.47
3 22	9.74	41.4	10.39	31 2	6.42	21.5	8.59
				Feb. 4 22	7.64	38.7	8.50
April 30 22	8.97	50.6	8.92	12 2	6.93	32.6	8.25
May 1 8	9.77	64.1	8.70	16 23	7.49	38.9	8.33
1 22	9.28	56.3	8.80				
2 21	9.13	55.2	8.73	Mar. 9 22	7.43	41.0	8.11
3 8	9.81	64.4	8.72	12 23	6.87	31.6	8.27
3 23	9.02	53.6	8.75	13 22	7.08	31.9	8.46
				16 22	6.82	31.2	8.25
				23 23	8.07	45.6	8.40
				April 1 11	8.10	48.7	8.20

It should be remarked, that the series for January 1844 is not comparable with the following series, as an adjustment of the instrument occurred in that month; neither, indeed, are the other series comparable with each other, from the circumstances given in Table I.

7. To take one of the most marked cases from this table, it will be seen that the observed times of vibration on January 23d and 31st 1845, differ nearly two seconds, while the corrected times do not differ one-tenth of a second.

8. While an inequality in the expansion of some parts of the needle would alter its sensibility by elevating the centre of gravity, it seems very doubtful if there is any thing in the form of the needle which is at all likely to render this supposition sufficient. An alteration in the position of the centre of motion would produce a like effect; and as the position of the needle depends, to some extent, on its temperature, it is necessary to shew whether position or temperature only is the cause of the differences in the times of vibration. Had the readings for the position of the needle been given with Table III., it would have been evident from these alone that the differences were *not* due to differences of posi-

tion. The following series of observations made during a magnetic disturbance, will, however, prove it more distinctly.

TABLE IV.

Observations for the Time of Vibration of the Balance Needle in a Vertical Plane, the position of rest varying.

Gottingen Mean Time of Observation.	Balance Magnetometer.		Time of one Vibration.	
	Reading.	Thermometer.	Observed.	Corrected to 50° Fahr.
d h m	Mic. Div.	°	s	s
April 15 22 52	—148	47.7	8.84	9.01
17 1 50	+101	52.5	9.21	9.02
2 20	+ 4	53.2	9.46	9.22
3 15	+ 25	54.2	9.46	9.14
3 25	+ 23	54.7	9.36	9.00
5 15	— 18	56.4	9.62	9.13
7 45	— 10	56.3	9.66	9.18
8 45	— 90	56.0	9.68	9.22
10 20	—190	55.2	9.42	9.02
10 30	—176	55.2	9.62	9.22
13 40	—310	54.3	9.23	8.90
13 50	—293	54.3	9.35	9.02
22 15	—185	51.1	9.28	9.20
22 25	—185	51.1	9.38	9.30
18 22 30	—134	50.3	9.01	8.99

The positive and negative signs indicate that the north pole of the needle was below or above the horizontal. It would have required a change of 50° Fahr. to have produced *alone* a difference of 400 micrometer divisions. Such a change of temperature, according to § 6, would have been equivalent to a change of $3^{s}.8$ in the time of vibration. The observed times differ only a few tenths, and the times corrected for temperature agree within the limit of the errors of observation.*

9. It results from these facts, that the time of vibration in a vertical plane cannot be used at present in the reduction of the observations, as theory takes no account of them. The theoretical corrections for differences of arc or the variation of the moment of inertia due to temperature would, in the examples given, be inappreciable.

10. I shall now consider $\frac{\Delta m}{m}$, the temperature correction for the position of the needle.

The method which has been adopted for its determination is as follows:—

The magnet, whose temperature correction is to be obtained, is placed at right angles to a magnet freely suspended, which is thus deflected by an angle u from the magnetic meridian. If m be the magnetic moment of the deflecting magnet, and X the horizontal component of the earth's magnetic force, the equation of equilibrium is

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* The time of vibration throughout the year varies from other causes. The law which regulates these variations I have not yet determined.

$$m = X \sin u \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

The variations of u are observed, while the deflecting magnet has its temperature altered 30° or 40° Fahr., by means of hot or cold water; by differentiating equation (4) and dividing by it, these variations are connected with $\frac{\Delta m}{m}$ by the equation.

$$\frac{\Delta m}{m} = \cot u \, \Delta u \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

X and the magnetic declination being constant.

11. The chief objections to this method are the following:—

1*st*, The circumstances under which the magnet is placed are considerably different from its usual condition. It is necessary to raise or lower the temperature 30° or 40° in water, within a few minutes, to obtain satisfactory results, whereas the most rapid changes in the magnetometer-box will probably be under 2° in an hour. It seems doubtful to me whether it has been proved that the changes of magnetic moment occur as rapidly as those of temperature in all cases.

2*d*, In the event of there being any other source of error due to temperature, it is altogether omitted by this method.

3*d*, If the correction has not been determined before adjusting the instrument, the series of observations is broken up by the necessity of removing the needle.

12. As it is desirable that the observations of the balance magnetometer should be made as valuable as possible, I shall proceed to consider how this may be best done, as it is my opinion that they will be found ultimately capable of giving diurnal and annual changes with considerable fidelity.

13. The observations of $\Delta \epsilon$, the varying angle formed by the needle and the horizontal, will at present obviously give comparative observations for the variations of vertical force, without reference to the value of the coefficient $\tan \epsilon$, until a good approximate value of the latter can be obtained, *if* the observations in micrometer divisions can be corrected for temperature. In order to do this, it would be necessary to convert the value of $\frac{\Delta m}{m}$, obtained by deflection experiments into micrometer divisions, if this value be q.

$$q = \frac{\Delta m}{m} \tan^2 \theta \, \frac{T^2}{T'^2} \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

We cannot, however, use T, and therefore the method of deflections is, in this way, insufficient; besides, if the alterations in the value of T from temperature should be caused by changes in the position of the centre of gravity, this change would probably not be altogether in the vertical, the portion resolvable to the horizontal would affect the *position* of the needle.

14. From these considerations I was induced, about two years ago, to endeavour to obtain the temperature correction from the usual daily observations of the instrument. To most persons acquainted with the irregularities in the magnetical variations, from the changes of the magnetic intensity or its direction, this might appear to some extent chimerical, and as at best only capable of giving a rough approximation to, or verification of, the determinations by deflection. It will, however, I think, be shewn, that a better coincidence of partial results, and a better correction, may be obtained from this than from the usual method.

It will not be necessary to point out the methods which were at first tried; I shall proceed at once to those which have been ultimately adopted.

15. Having selected a series of days during which the readings of the instrument seem regular, and in which the changes of temperature from day to day are considerable, rejecting any day of marked disturbance, the hourly or two-hourly readings for the position of the needle and for its temperature are summed for each day. Let us designate the sum of the micrometer readings for the first day of the series y_1 for the second day y_2, and so on to $y_{2\overline{n+1}}$; the corresponding sums of the thermometer readings being t_1, t_2, ... $t_{2\overline{n+1}}$, the number of the days, from the beginning to the end of the period, being $2\overline{n+1}$.

The most simple and probable hypothesis that can be formed, is, that the mean vertical force increases or diminishes gradually throughout the period; let the mean daily change be a.

If q be the temperature correction for 1° Fahr. in micrometer divisions, we may form the following series of equations:

$$\left.\begin{array}{ll}
y_1 = y_2 + \ a \ + (t_1 - t_2)\,q & y_2 = y_3 + \ a \ + (t_2 - t_3)\,q \\
y_1 = y_3 + 2\,a + (t_1 - t_3)\,q & y_2 = y_4 + 2\,a + (t_2 - t_4)\,q \\
\cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot & \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot \\
y_1 = y_{n+1} + n\,a + (t_1 - t_{n+1})\,q & y_2 = y_{n+2} + n\,a + (t_2 - t_{n+1})\,q \\
\multicolumn{2}{c}{\cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot} \\
\multicolumn{2}{c}{y_{n+2} = y_{\overline{n+3}} + a + (t_{n+2} - t_{\overline{n+3}})\,q} \\
\multicolumn{2}{c}{\cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot}
\end{array}\right\}(7.)$$

There will be breaks in each series, as there are no sums for the Sundays. As t_2 may be greater than t_1 and t_2, the result of the comparison of y_1 with y_3 is not equivalent to the comparison of y_1 with y_2 and y_2 with y_3.

From these equations the most probable values of a and q might be obtained by the usual methods; but the labour which they demand is probably much beyond the greater accuracy to be attained. The following, it is conceived, will be found sufficient.

First classing the equations in which $t_p >$ or $\angle\, t_{p+r}$, and considering each class separately.

Placing the equations in the form

$$\frac{y_p - y_{p+r}}{t_p - t_{p+r}} = \frac{r\,a}{t_p - t_{p+r}} + q \quad . \ . \ . \ . \ . \quad (8.)$$

Naming the differences in which $r = 1$, Δy_1, and Δt_1, in which $r = 2$, Δy_2, and Δt_2 . . . . Δy_n, Δt_n. Summing separately all the equations for Δ_1, all those for Δ_2 . . . . . It will simplify the investigation, and be sufficiently accurate to take for the divisor of $r\,a$, the mean of all the values of Δt, naming this Δt_0.

We obtain the following equations:

$$\left.\begin{aligned} \frac{\Sigma\Delta y_1}{\Sigma\Delta t_1} &= q + \frac{a}{\Delta t_0} \\ \frac{\Sigma\Delta y_2}{\Sigma\Delta t_2} &= q + \frac{2\,a}{\Delta t_0} \\ . \ . \ . \ . \ &. \ . \ . \ . \\ \frac{\Sigma\Delta y_n}{\Sigma\Delta t_n} &= q + \frac{n\,a}{\Delta t_0} \end{aligned}\right\} \quad (9.)$$

If the difference of each equation be taken with every one following it, another series of equations of the following form will be produced.

$$\frac{\Sigma\Delta y_p}{\Sigma\Delta t_p} - \frac{\Sigma\Delta y_{p+r}}{\Sigma\Delta t_{p+r}} - \frac{r\,a}{\Delta t_0} \quad . \ . \ . \quad (10.)$$

Summing the equations thus formed, we obtain an equation which may be put as follows:

$$\overline{n-1}\left(\frac{\Sigma\Delta y_1}{\Sigma\Delta t_1} - \frac{\Sigma\Delta y_n}{\Sigma\Delta t_n}\right) + \overline{n-3}\left(\frac{\Sigma\Delta y_2}{\Sigma\Delta t_2} - \frac{\Sigma\Delta y_{n-1}}{\Sigma\Delta t_{n-1}}\right) + \ . \ . \ . \ = -\frac{\overline{n+1}\,.\,n\,.\,\overline{n-1}}{6}\cdot\frac{a}{\Delta t_0} \quad (11.)$$

Summing equations (9.)

$$q = \frac{1}{n}\Sigma\frac{\Sigma\Delta(y)}{\Sigma\Delta(t)} - \frac{n+1}{2}\,\frac{a}{\Delta t_0} \quad . \ . \ . \ . \ . \ . \quad (12.)$$

16. The following example, from the Makerstoun observations, will shew the method found most convenient in practice for the summations.

A period of 52 days, from June 1 till July 22. 1843, having been selected as nearly free from disturbances, and containing considerable changes of temperature, the 3d and 7th June being rejected on account of disturbances; the sums for each day of the micrometer and thermometer readings were entered into columns titled Σy and Σt. Each sum was then compared with all the sums up to the 27th day after, and the differences entered into columns titled Δy_1, Δt_1; Δy_2, Δt_2; . . . . Δy_{26}, Δt_{26}. Those differences, the fewest in number, in which $t_p > t_{p+r}$ were marked out, the others summed for each column, and the divisions $\frac{\Sigma\Delta y_1}{\Sigma\Delta t_1}$ . . . . $\frac{\Sigma\Delta y_{26}}{\Sigma\Delta t_{26}}$ performed.

From these and equations (11), (12),

$$\Sigma\frac{\Sigma\Delta(y)}{\Sigma\Delta(t)} = 8.338\,; \quad \frac{a}{\Delta t_0} = 0.0375\,; \quad a = 2.05\,; \quad q = 7.832 \text{ Mic. div.}$$

The differences, when $t_p > t_{p+r}$ were too irregular and too few, on some days, to give a good value of a.

17. It is very rare that periods of such magnitude can be found free from considerable irregularities. In general, however, it is conceived that smaller periods will give equally good results, and by a shorter method.

If we consider the equations

$$q = \frac{y_p - y_{p+r}}{t_p - t_{p+r}} - \frac{r\,a}{t_p - t_{p+r}}\,; \qquad t_p > t_{p+r}$$

$$q = \frac{y_{p+r} - y_p}{t_{p+r} - t_p} + \frac{r\,a}{t_{p+r} - t_p}\,; \qquad t_p < t_{p+r}$$

it is obvious, that if the temperatures rise and fall considerably throughout the period selected, and no attention be paid to the sign of $t_p - t_{p+r}$ in the summations of the differences, the coefficients of a will nearly destroy each other.

18. In the following cases the sums for each day have been compared with the sums of all the days after it in the period selected. By this means irregularities in the force upon any day have their effect on the final result to a considerable extent destroyed, as it is probable that the results will be as much too great in some cases, as they are too small in others.

The whole differences have been summed without regard to days, and the signs of $t_p - t_{p+r}$ have been disregarded.

The equation is, therefore, simply

$$\frac{\Sigma\,\Delta\,(y)}{\Sigma\,\Delta\,(t)} = q$$

TABLE V.

Determinations of the Temperature Correction for the Balance Magnetometer, from comparisons of the Daily Observations at different periods.

PERIOD.	$\Sigma\,\Delta\,(t)$	$\Sigma\,\Delta\,(y)$	q	Time of Vibration corrected to 50° Fahr.	REMARKS.
1843.	°	Mic. Div.	Mic. Div.	s	
Jan. 16—21.	525.3	4315.3	8.21	9.20	In 1843, there were 9 daily observations made at two-hourly intervals, from 5 A.M. till 9 P.M.
23—28.	817.7	5723.5	6.99	9.20	
Jan. 30—Feb. 4.	576.0	4151.5	7.21	9.02	
Feb. 6—11.	609.9	4080.6	6.69	9.25	Sept. 2, the needle was removed, in order to determine its temperature correction by the method of deflections.
June 1—30.	14320.4	114646.9	8.006	9.28	
Sept. 6—16.	1083.7	8730.4	8.04	9.92	
1844.					
May 9—24.	8415.4	66621.7	7.93	8.38	In 1844, there were observations at every hour of the day.
Aug. 3—Sept. 6.	21696.9	171460.5	7.902	8.06	
For the series in 1843.	17933.0	141648.2	7.898		The needle was removed between September and February for temperature correction deflections.
For all,	48045.3	379730.4	7.903		

From the above table it would appear that neither the removal of the needle and readjustment, nor the alteration of the time of vibration, has affected the temperature correction.

The first values of q shew that periods of a week are insufficient for very accurate determinations.

The mean for 1843 is almost identical with that for 1844.

19. The differences for three periods were also summed without regard to days, but paying attention to the sign of $t-t_{p+r}$. The following table contains the results.

TABLE VI.

Determination of the Temperature Correction for the Balance Magnetometer, regard being paid to the signs of the differences of temperature.

PERIOD.	Preceding temperatures greater than the succeeding.			Preceding temperatures less than the succeeding.			Mean Value of q
	$\Sigma\Delta(t)$	$\Sigma\Delta(y)$	q	$\Sigma\Delta(t)$	$\Sigma\Delta(y)$	q	
	°	Mic. Div.	Mic. Div.	°	Mic. Div.	Mic. Div.	Mic. Div.
1843. June 1—30.	3350.2	29096.6	8.68	10970.2	85550.3	7.80	8.24
1844. May 9—24.	5404.3	37559.9	6.95	3011.1	29061.8	9.65	8.30
Aug. 3—Sept. 6.	4726.1	34249.3	6.68	16970.8	137211.2	8.09	7.39
For all the periods,	13480.6	100905.8	7.49	30952.1	251823.3	8.14	7.813

The result No. 16, and the mean results in Tables V. and VI., for the whole periods, agree very closely. As the value of one micrometer division, in parts of the whole vertical force, is about 0.00013, the greatest difference of the three final results, 7.83, 7.90, and 7.81, is 0.0000012.

The final results, from five days' observations, by the method of deflections, were .000085, .000077, .000079, .000062, .000073, differing 0.000023.

The results, from the comparison of daily observations, in parts of the whole vertical force, will be about .000134, the time of vibration being about 9 seconds; if 11 seconds were adopted, the result would be .000095; in either case considerably more than the result obtained by deflections.

20. The satisfactory determination of q for the Balance magnetometer, led me to determine the correction for the Bifilar magnetometer by the same method.

Besides the variation of the magnetic moment, temperature also affects the length and interval of the suspending silver wires; it probably also affects their elasticity.

The determination of the correction from the daily observations, at once sums up all the effects of temperature. When the suspending threads are of silk, these sources of error are avoided; but I conceive that much graver errors are introduced, due chiefly to varying humidity affecting the torsion of the thread.

I shall give simply the results of the comparisons of the daily observations for the Bifilar magnetometer.

TABLE VII.

Determination of the Temperature Correction for the Bifilar Magnetometer, from comparisons of the Daily Observations.

PERIOD.	Preceding temperatures greater than the succeeding.			Preceding temperatures less than the succeeding.			Mean of the two values of q in parts of force.	Value of q independent of the sign of $\Delta(t)$ in parts of force.
	$\Sigma\Delta(t)$	$\Sigma\Delta(x)$	q	$\Sigma\Delta$	$\Sigma\Delta(x)$	q		
1844.	°	Sc. Div.	Sc. Div.	°	Sc. Div.	Sc. Div.		
May 9—24.	5334.9	13066.8	2.45	2359.9	4033.0	1.71	0.000270	0.000289
May 29—June 28.	11938.2	24597.2	2.06	26719.2	45966.3	1.72	0.000246	0.000238
July 17—30.	1843.1	3004.0	1.63	4637.8	8470.4	1.83	0.000225	0.000230
Sept. 2—25.	27322.6	53684.3	1.96	622.1	1260.8	2.03	0.000259	0.000255
Nov. 26—Dec. 13	17855.4	36791.6	2.06	2143.7	3104.5	1.45	0.000229	0.000259
For all the periods,	64294.2	131143.9	2.04	36482.7	62835.0	1.72	0.000244	0.000250

When it is considered that the daily range of the Bifilar readings, in parts of the whole horizontal force, is to the daily range of the Balance readings, in parts of the whole vertical force, as 7 or 8 to 1, it will be seen that the results for the Bifilar magnetometer are equal to those for the Balance. It should also be remembered that the results for May and July are from short periods.

The results obtained by deflections on two days were 0.000291 and 0.000298, the partial results agreeing very well.

Taking into account the expansion of the wires, the total temperature correction is 0.000304.

It will be observed, in this case, that the results by deflections are greater than those from a comparison of the daily observations.

MAKERSTOUN, *April* 18. 1845.

X.—*On Wollaston's Argument from the Limitation of the Atmosphere, as to the Finite Divisibility of Matter.* By George Wilson, M.D., *Lecturer on Chemistry.*

[Read 21st April 1845.]

In the year 1822, Dr Wollaston published a remarkable paper "on the finite extent of the atmosphere."* Its object is to establish, by observations on the motions of certain of the heavenly bodies, that our atmosphere does not extend into free space, and to deduce from this limitation in its extent, the conclusion, that the air necessarily consists of particles "no longer divisible by repulsion of their parts;" *i. e.* of true atoms. From this there is the further inference, that, "since the law of definite proportions discovered by chemists, is the same for all kinds of matter, whether solid or fluid, or elastic, if it can be ascertained that any one body consists of particles no longer divisible, we then can scarcely doubt that all other bodies are similarly constituted." In other words, the existence of a limit to the earth's atmosphere is declared to supply a demonstration of the finite divisibility of matter.

In pursuing this argument, Wollaston first discusses the question, What is the probable height to which the earth's atmosphere extends? And after stating, that, from the known laws of the elasticity of the atmosphere, we should infer that it extends to the height of 40 miles, with properties yet unimpaired by extreme rarefaction, he proceeds to say, "Beyond this limit we are left to conjectures founded on the supposed divisibility of matter; and if this be infinite, so also must be the extent of our atmosphere. But if air consist of any ultimate particles no longer divisible, then must expansion of the medium composed of them cease at that distance where the force of gravity downwards, upon a single particle, is equal to the resistance arising from the repulsive force of the medium." Wollaston, it will be observed, takes for granted two things. 1*stly*, He assumes that the law which is known to connect the density of the air with the compressing force, near the surface of the earth, prevails, without change, to the limit of the atmosphere. 2*dly*, He identifies the divisibility of the mass with that of its component parts or molecules. If the molecule be infinitely divisible, the mass will be so also, and *vice versa;* so that if the divisibility (finite or infinite) of either be ascertained, that of the other will thereby be ascertained also. Now, the atmosphere is not merely divisible, but, consisting like other gases of mutually repulsive particles, contains within itself a power of division. We have only, therefore, to permit this self-dividing force to come into play, and the result, according to WOLLASTON, will shew whether the mass undergoing spontaneous division is infinitely divisible or not. This experiment we cannot try; but it has long ago been performed for us by the hand of Nature. Our atmosphere has divided itself to the utmost limit which its susceptibility of division permitted, and has thereby tested or ascertained that divisibility for us. Either that is infinite, in which case, the atmosphere must have spread into space, and portions of it will be found surrounding the different heavenly bodies, varying in amount according to their respective dimensions, temperatures and the like. Or it is finite, and the air has found a limit at no great distance from the earth; for the particles of which it consists, although free, so far as their mutual repulsiveness is concerned, to recede from each other, are not equally free to recede from the earth, to which the force of gravitation binds them. They must come to rest accordingly at the point where the attraction of gravitation is equal in amount, while it is opposite in direction to the force of repulsion among them; so that they are balanced in equilibrio between them. Now it appears on making the necessary observations, that probably the Sun, and certainly that Jupiter, is devoid of an atmosphere of the same nature as our own. Therefore, concludes WOLLASTON, our atmosphere is of finite extent, and consists of particles only finitely divisible. And as the air cannot be supposed to be peculiar in this respect, the conclusion is immediately extended to every other substance, and all matter is inferred to consist of finitely divisible particles, or *bonâ fide* atoms.

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* Philosophical Transactions, 1822, p. 89.

It cannot surprise us that so remarkable a paper as WOLLASTON's should have excited the greatest attention among men of science. If the argument pursued in it were just, the vexed question of the finite or infinite divisibility of matter, which, for some thousand years, physics and metaphysics had alike sought in vain to decide either way, had all the while been answered for us. Every attempt towards the solution of that problem by experiment had failed, not perhaps, because ultimate atomic particles had not been arrived at, by the dividing forces our command, (for this length the inquiry never reached); but because, long before the divisibility of a body could be supposed to be exhausted, the products of its division had become invisible to us, and we had no test by which to tell when the atoms of a substance had been attained to. Both of these difficulties, according to WOLLASTON, were taken out of the way, by the mode in which Nature made the experiment. A dividing force co-ordinate with the divisibility on which it took effect—finite, if it were finite, infinite, if it were infinite—was brought into play. The result of this division, moreover, could be ascertained, could, in truth, literally be seen; for it did not take place in a vacuum, but in a space containing bodies, each of which would infallibly indicate the extension of a self-dividing medium at least to itself; and as that, if it were infinitely divisible, must reach to them all in its progress towards infinite division, should it certainly appear that it had not extended to any one, still more, if not to several, it would suffice to prove that it was not infinitely divisible. In short, our atmosphere being the self-dividing mass, and all the stars standing between it and infinity, the absence of an atmosphere like the earth's, from any one of them, shews that that it can only be finitely divided; and the decision in the negative of the question of infinite divisibility should have dated from the discovery of the telescope, and GALILEO's earliest observation of the eclipses of Jupiter's moons.

My object in the following remarks, is to shew that WOLLASTON's identification of the divisibility of the molecule, with the observed division of the mass of which it is a part, is altogether unwarrantable; that he takes for granted the very thing to be proved; and that his whole discussion leaves the question of the finite or infinite divisibility of matter exactly where it found it. Before doing so, however, I am anxious to refer very briefly to the criticisms already offered on this part of the paper under discussion.

The opinions hitherto expressed as to WOLLASTON's argument may be arranged, I believe, under four heads. 1*st*, A few natural philosophers have entirely assented to the truth of the conclusion contained in it. Among these was DAUBENY,* who has lately, however, withdrawn his assent;† and it is still advocated by DUMAS, who, whilst he objects to WOLLASTON's arguments, on other grounds which will be referred to immediately, appears to consider the conclusion of the latter unavoidable, if his premises are granted him.‡

2*d*, A greater number, including FARADAY,§ GRAHAM,‖ and TURNER,¶ have implied, by the terms of commendation in which they have referred to it, that at least they detected no fallacy in the argument.

3*d*, It has been objected to by DUMAS (following out the views of POISSON), on the ground that the low temperature which is known to prevail in the upper regions of the atmosphere, may be such at its boundary as to destroy the elasticity of the air, and even to liquefy or solidify it.* DAUBENY,† KANE,‡ and others, have replied to this, that the temperature of planetary space, according to FOURIER, SCHWANBERG, and others, is much higher than that to which air has been exposed in experiments with solid carbonic acid and ether, without destroying its elasticity. DUMAS, in anticipation of such objections, has declared, that we are not to consider the temperature which a thermometer would exhibit if placed in the upper strata of the atmosphere, as necessarily identical with that of the air around it.§ By which statement he means to enforce, if I understand him aright, that non-elastic (liquid or solid ?) air may, like other diathermanous bodies, transmit heat without being thereby raised in temperature itself, so that the outer shell of air may be colder than the layers within it, or space beyond it. In allusion to such a view, Professor JAMES FORBES has pointed out the difficulty of understanding "how it is possible that the higher strata of the atmosphere can remain permanently colder than the strata beneath and the sky above them, without admitting a paradox of the same kind with a mechanical perpetual motion."‖

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* Introduction to the Atomic Theory, 1831, pp. 103-5.

† Supplement to Introduction, &c., 1840, p. 11.

‡ DUMAS' assent was entirely negative, but was strongly manifested, and is the more remarkable, that he has directed special attention to the phenomena presented by those gases which combine without undergoing diminution of their volume, as irreconcilable with the idea of the chemical equivalents of these bodies being represented by single atoms, such as DALTON assumed, (Leçons sur la Philosophie Chimique, p. 263). Had this view been carried out and applied to the atmosphere, it would have struck at the root of WOLLASTON's whole train of reasoning, and would have obviated the necessity of appeal to the questionable views of POISSON, as to the cause of the limitation of the atmosphere. As WHEWELL's discussion of WOLLASTON's speculations, which was specially intended to meet the argument of DUMAS, has appeared since the latter published his views, it may have led to some modification of his opinion. But that distinguished chemist has not had occasion, so far as I am aware, to refer again to the subject in public; so that, in the meanwhile, I include him among the acknowledged supporters of the intrinsic validity of WOLLASTON's views.

§ On the existence of a limit to vaporisation, Phil. Trans., 1826, p. 492.

‖ Elements of Chemistry, pp. 68 and 273.

¶ Elements of Chemistry, 7th edition, p. 207.

In reference to DUMAS' mode of disposing of WOLLASTON'S argument, I would only further observe, that natural philosophers are not at one as to the temperature, either of planetary space or of the upper strata of the atmosphere; so that it is impossible at present to say what is the exact value of the objection I have been discussing.

4th, Finally, several physicists have denied the justness of WOLLASTON'S conclusion, on the ground of its intrinsic invalidity. Among these are Professor JAMES FORBES¶ and Dr KANE,** who have not, however, so far as I am aware, stated in what way they dispose of the argument. Professor WHEWELL is likewise an objector, and dissents from WOLLASTON'S inference, on the plea that the latter was not at liberty to assume that the law which connects the density of the air with the compressing force at the upper boundary of the atmosphere, is identical with that which is known to prevail near the earth. His own words are—"We know nothing of the law which connects the density with the compressing force in air so extremely rare, as we must suppose it to be near the boundary of the atmosphere. Now there are possible laws of dependence of the density upon the compressing force, such that the atmosphere would terminate in virtue of the law without any assumption of atoms. This may be proved by mathematical reasoning. If we suppose the density of air to be as the square root of the compressing force, it will follow that, at the very limits of the atmosphere, the strata of equal thickness may observe in their densities such a law of proportion as is expressed by the numbers 7, 5, 3, 1. For the compressing force on each being as the whole weight beyond it, will be for the four highest strata 16, 9, 4, and 1, of which the square roots are as 4, 3, 2, 1, or as 8, 6, 4, 2; and, though these numbers are not exactly as the densities 7, 5, 3, 1, those who are a little acquainted with mathematical reasoning will see that the difference arises from taking so small a number of strata. If we were to make the strata indefinitely thin, as to avoid error we ought to do, the coincidence would be exact; and thus, according to this law, the series of strata terminates as we ascend, without any consideration of atoms."*

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* Leçons, &c., p. 239.

† Supplement to the Introduction to the Atomic Theory, p. 11.

‡ Elements of Chemistry, p. 441.

§ Leçons, &c., p. 241.

‖ Report of British Association, 1841, p. 79.

¶ Op. cit., p. 77.

** Elements of Chemistry, pp. 15 and 358.

My object in the succeeding argument is to shew, that, although the law which WOLLASTON assumed to prevail in the higher regions of the atmosphere were in operation, it would not justify the conclusion which he supposed it to warrant. The discussion which follows differs from WHEWELL's mode of disposing of the subject, in conceding to WOLLASTON his own law; and from that of POISSON and DUMAS, in permitting him to take for granted as high a temperature as he pleases, provided only the atmosphere have reached a limit.

On a little consideration of WOLLASTON's reasoning, it will appear, that all that he succeeded at the utmost in establishing was, that the atmosphere consists of a finite number of repelling molecules. He seems to have conceived that this was sufficient, and that no one would dispute his subsequent assumption, that these repelling molecules were ultimate particles or true atoms.

But such an assumption is, on a twofold ground, inadmissible. The more important chemical components of our atmosphere are, water-vapour, carbonic acid, oxygen, and nitrogen. Let us set aside for the time, as we are at liberty to do, the influence of the low temperature of the upper regions of the air in condensing the water, and perhaps also the carbonic acid; and suppose our atmosphere, with a temperature at its boundary sufficient to retain all its constituents as elastic fluids, to find a limit, in virtue of the prevalence of WOLLASTON's law. Each gas would cease to expand for the same reason, and present a row of bounding molecules, which were prevented from falling towards the earth by the repulsion of the particles between it and them, and from receding from the earth by their own weight. But the molecules of water-vapour, and carbonic acid, brought in this way to a stand, would certainly not be ultimate particles or indivisible atoms. The molecule of water, on the simplest view of its constitution, namely, that the chemical equivalent corresponds to a single atom, would consist of at least two atoms, one of hydrogen and one of oxygen, with a centre of repulsion common to both. The molecule of carbonic acid, for similar reasons, would consist of three atoms, one of carbon and two of oxygen. And who shall assure us that oxygen and nitrogen are not compounds with binary chemical molecules like those of water, or ternary ones like those of carbonic acid? It is true that the chemist names these gases simple substances. But the simplicity he attributes to them is only, as he is careful to define, *quoad analysis*; and the physicist is not at liberty to convert this negative and relative simplicity into an absolute one, and make deductions therefrom, as WOLLASTON has done. If we argue from analogy, in reference to this point, we should infer that oxygen and nitrogen are compounds; for we know a much greater number of gases in which the molecule is a group of chemically distinct atoms, than we do of elastic fluids, where, on the most favourable view, it can be supposed to be a single one. But it is not necessary to pursue any argument of this kind; nor is the objector called upon to shew that oxygen and nitrogen are chemical compounds. It is sufficient for his purpose to decline assent to WOLLASTON's conclusion till he, or those who agree with him, supply proof that the molecules of oxygen and of nitrogen are chemically simple. The *onus probandi* clearly lies, not with the denier but with the asserter, of a positive proposition like the one before us.

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* Philosophy of the Inductive Sciences, vol. i., p. 420, and Athenæum, 1839, pp. 724-7.

In so far, then, as WOLLASTON assumed the chemical simplicity of two of the gases of the atmosphere, he employed an *argumentum ad ignorantiam*. He was guilty also of a *petitio principii*. For even, if it could be shewn, that oxygen and nitrogen are chemically homogeneous, and do not, on that account, admit of comparison as to the constitution of their gaseous molecule with water and carbonic acid, it would not warrant the conclusion, that that molecule was an atom. Does it follow as a necessary inference, that because a body is simple, its gaseous repelling molecule must consist of but one atom? The answer is assuredly in the negative. The molecule might, on the other hand, be made up of a pair of atoms, like a binary star, with a centre of repulsion common to the two; or of 10, or of 100, or 1000 atoms (if such bodies there be), grouped together into a compound whole. We have no means whatever, in truth, of estimating what the complexity of the molecule may be. Without insisting at greater length on this, it is at least manifest, that we are not even at liberty to identify the combining chemical molecule with the repelling gaseous one, much less to identify either with the ultimate atom. Yet, unless WOLLASTON was at liberty to do so, his argument was useless towards settling the question of the divisibility of matter. To prove that the atmosphere consisted of finite molecules, was only to reach the threshold of the difficulty; for each molecule supplied as good a text whereon to dispute the question of infinite divisibility, as the whole atmosphere out of which it was taken. The point which most of all demanded proof, namely, that the molecule was an atom, was the very one which he took for granted.

WOLLASTON, in truth, erred, in assuming that the self-dividing power present in the atmosphere was able to divide, to the uttermost, the divisible mass subjected to its action; in taking for granted that the divisibility was co-ordinate with the actual division, so that the latter was the exact index and measure of the amount of the former. The fallacy of his argument will at once appear if the latter be thrown into a syllogistic form. It will then run thus:—

1. An atmosphere consisting of an infinite number of mutually repulsive particles, must be infinitely extended.

2. But our atmosphere is not infinitely extended.

3. Therefore our atmosphere does not consist of an *infinite number of particles*. Whereas it should have been.

Therefore our atmosphere does not consist of an infinite number of *mutually repulsive* particles.

The premises fully warrant the conclusion that our atmosphere does not consist of an infinite number of *mutually repelling* particles, but throw no light on the question, whether or not it may contain an infinite number of *mutually indifferent*, or *mutually attractive* ones.

WOLLASTON'S argument, then, supplies no decision of the question of the divisibility of matter. That problem still presents the same twofold aspect of difficulty which it has ever exhibited. If we affirm that matter is infinitely divisible, we assert the apparent contradiction, that a finite whole contains an infinite number of parts. If, pressed by this difficulty, we seek to prove that the parts are as finite as the whole they make up, we fail in our attempt. We can never exhibit the finite factors of our finite whole; and the so-called atom always proves as divisible as the mass out of which it was extracted. Finity and infinity must both be believed in; but here, as in other departments of knowledge, we cannot reconcile them.

It seems surprising that fallacies so palpable as those we have been discussing, should not have been detected long ago by the able philosophers who have noticed WOLLASTON'S argument. It is especially singular, that DUMAS, who holds that, in the combination of gases, a division of the chemical equivalent frequently occurs (so that he represents the latter as expressed physically by a group of many molecules), should not have applied his views, as he could so easily have done, to its full refutation.

As it is, I trust that the discussion I have laid before the Society will not prove unacceptable to its members. WHEWELL'S reasoning cannot be appreciated by those who are ignorant of mathematics; and the views of POISSON and DUMAS, even should they be fully established, leave unconsidered the question of the intrinsic validity of WOLLASTON'S conclusions. I am not without hopes, accordingly, that a demonstration of a fallacy in the argument in question, on purely physical grounds, which can be understood by every one, and which, so far as I am aware, has not been offered already, will be of service in removing doubtful knowledge. In particular, it may save beginners from seeking for a demonstration where none is to be found, and from blaming themselves because they cannot acquiesce in a conclusion, the truth of which great names have appeared to warrant. I would, observe, however, that, although I have employed the words *molecule* and *atom* in the preceding discussion, it has been for the sake of simplicity and convenience, and to meet WOLLASTON on his own ground. I do not wish to be understood as offering any opinion as to the ultimate constitution of matter, except in so far as I deny the success of the only attempt which has been made in modern times, to establish, by direct observation, the existence of indivisible atoms.

17 MR 58